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EXTINCTION AS A FUNCTION OF THE SPACING OF EXTINCTION TRIALS [1]

WALTER C. STANLEY [2]
Brown University

Most reviews (e.g., **5, 15, 20**) and most theoretical statements (e.g., **7, 10, 11, 12**) imply that massed extinction trials will produce more decrement in a learned response than spaced extinction trials. A recent study by Sheffield (**19**), however, obtained directly opposite results. Rats trained to run down an alley to obtain food clearly extinguished more rapidly with spaced extinction trials (one trial every 15 min.) than with massed extinction trials (15 sec. between trials). Although this result runs counter to internal and response-produced inhibition theories of extinction which imply that spacing of extinction trials should allow time for the response to recover "spontaneously," it is not entirely unexpected if other findings are considered. If we ignore studies based on only one *S* (**4, 12**), five studies (**1, 2, 16, 17, 18**) show the expected faster extinction with massed trials, while two (**13, 14**) show trends in the opposite direction. The present research represents an attempt to reconcile the divergent findings on this important topic.

First, there is some question as to whether the Sheffield finding implies that resistance to extinction of the running habit was greater in her massed than her spaced *S*s, or whether the difference she obtained reflects merely differential drive strengths in the two groups. Suppose we assume, with Sheffield, that omission of food during extinction is frustrating and produces a temporary drive state; and that during massed extinction more of this frustration-produced drive carries over from the end of one trial to the beginning of the next than during spaced extinction. Then we can say either: that the massed *S*s ran faster because more of the frustration-produced drive was "channelized" into the running habit (**19**); or, that the massed *S*s got to the goal box sooner because they were more excited and active and had nowhere to

[1] This article is based on a dissertation submitted to the faculty of the Department of Psychology of Yale University in partial fulfillment of the requirements for the Ph.D. degree. The author wishes to express his gratitude and indebtedness to the members of his thesis committee, to Dr. Fred D. Sheffield under whose direction this research was conducted, and to Dr. Neal E. Miller and Dr. Frank A. Beach who served as advisors.

[2] A preliminary report of this research was presented at the 1950 meetings of the APA.

go except to the goal box. Clearly, no certain inference can be drawn concerning resistance to extinction of the running habit.

Second, several studies (**1, 13, 14**) in this area did not control adequately for the possible decremental effects of changing from one intertrial interval during acquisition to another during extinction. Theoretically, we should expect that such changes, either from massed to spaced trials, or vice versa, should make the stimulus pattern present at the start of each extinction trial somewhat different from the stimulus pattern present at the start of each acquisition trial. Since generalized responses are weaker to begin with and extinguish more quickly than "non-generalized" ones (**6**), extinction should be more rapid where such intertrial-interval changes occur than where the intertrial interval remains unchanged throughout. Presumably, such generalization decrement need not be limited to stimulus-intensity (**16**), but could be based on any aspect of the stimulus pattern correlated with duration of intertrial interval.

To clarify the theoretical import of the Sheffield result as well as to throw light on other divergent findings, the present research sought to determine the effect of the spacing variable on (*a*) the resistance to extinction where an alternative behavior route is provided—a situation where both vigor and correct response measures of extinction are available, and (*b*) the vigor and correct response measures of performance where extinction does not involve the frustrating effect of omitting a reward, but rather involves elimination of the primary drive. Finally, (*c*) it was of interest to determine whether a shift from massed training trials to spaced extinction trials, and vice versa, produced any generalization decrement.

To obtain data on (*a*) rats were trained to run a T-maze with food as a reward, and then given massed or spaced extinction by frustrating omission of the food. To obtain data on (*b*) rats were similarly trained, but then given massed or spaced extinction by running them after they had been satiated with food. Such a procedure presumably made omission of food nonfrustrating because reinforcement was prevented by elimination of the primary drive rather than by omission of the food reward.[3] Thus, any difference between massed and spaced extinction in terms of either running time or frequency of correct runs could be attributed to the spacing variable per se, and not to frustration.

To obtain data on (*c*), half of each extinction group received massed training trials and half received spaced training trials. This balanced out the possible effect of distribution of acquisition trials on rate of extinction and also provided data on generalization decrement.

Method

Subjects and apparatus

Male albino rats from the Charles River Breeding Laboratories were used. Forty *S*s were used in Part I and 64 in Part II of the study. The ages of *S*s ranged from 75 to 101 days (mean = 84) at the beginning of training. All *S*s were naive experimentally.

The T-maze used is shown in Fig. 1. The inside height was 11 in. Cord-operated doors prevented retracing. The starting box and goal boxes were covered with glass; screen

[3] Such nonfrustrating extinction operations, it should be noted, are comparable to those conventionally carried out to extinguish an escape response based on shock. Elimination of the primary hunger drive precludes reinforcement in the same fashion that omission of shock precludes reinforcement by removing the primary pain drive.

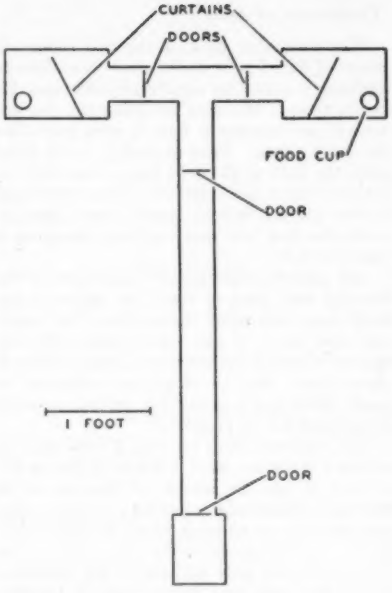

FIG. 1. The T-maze used

covered the remainder of the maze. A diagonal black curtain in each goal box prevented *S*s from seeing the food cup from the choice point. The interior of the starting box and the food cups were white; the rest of the apparatus was black. The light source was 5 ft. above the choice point.

Three Springfield timers controlled electrically by the movement of hinged floors gave three response time scores: (*a*) starting time, i.e., time from the raising of the starting door to *S*'s entry into the stem; (*b*) time required to reach the choice point, inclusive of starting time; and (*c*) total time required to reach either goal box.

Experimental design and procedure

Part I: Extinction by frustrating omission of food.—All *S*s received two weeks of taming; during the first week they were habituated to handling; during the second week they were allowed to explore and eat on a table top. Then two familiarization trials were given in the T-maze on the last day before the experiment proper. A wet mash pellet was placed in each food cup on this day, and eight pellets were distributed through the maze. Free exploration of both goal boxes was permitted, but otherwise no retracing was allowed. If *S* failed to eat either pellet within 10 min., it was discarded. There were five such discards. Forty *S*s remained for the experimentation in Part I.

The 40 *S*s were randomly assigned to the four groups shown in Table 1, and were further divided into equal groups for Subexperiments I and II. Within each group of ten *S*s, equal numbers learned to turn to the left and to the right in the maze, and the order in which all *S*s ran in the maze was randomized. During massed training or extinction, *S* was retained in an individual cage for 15 sec. between successive trials; during spaced training or extinction, *S* was placed in the starting box every 15 min.

The first five *S*s in each group received their 30 massed or spaced acquisition trials on experimental Day 1 and 30 massed or spaced extinction trials on Day 2 (Subexperiment I). The second five *S*s in each group received ten massed trials on Day 1, 30 massed or spaced acquisition trials on Day 2, and 30 massed or spaced extinction trials on Day 3 (Subexperiment II). This modification of procedure was introduced because one *S* in the first 20 failed to run after a few spaced acquisition trials and had to be replaced. It was believed that the ten preliminary massed trials would preclude possible differential elimination of slow learners in the two acquisition groups.

The reward used was a pellet of ground Purina Dog Chow mixed with water (.08 gm. dry weight). Each *S*'s total daily ration was brought to 7 gm. by a supplementary feeding which came at least 1.5 hr. after *S*'s last trial of the day, and 18 hr. before the midpoint of *S*'s training session on the next day. This schedule

TABLE 1

EXPERIMENTAL DESIGN IN PART I

Group	Subexperiment I*		Subexperiment II*		
	Training	Extinction	Training		Extinction
	Day 1	Day 2	Day 1	Day 2	Day 3
Massed-massed	30 M	30 M	10 M	30 M	30 M
Massed-spaced	30 M	30 S	10 M	30 M	30 S
Spaced-massed	30 S	30 M	10 M	30 S	30 M
Spaced-spaced	30 S	30 S	10 M	30 S	30 S

* $N = 5$ in each group.

served to equate hunger drive between the massed and spaced conditions.

On each trial *S* was placed in the starting box, facing the door, which was raised 1 sec. after *S*'s feet touched the floor. When *S*'s body cleared the starting door, the door was lowered. The choice point and arm doors were lowered as *S* stepped on the respective hinged floors. The *S* was retained for 10 sec. in the goal box it entered, sometimes longer in the correct goal box to allow time for the food to be eaten.

The procedure during extinction was the same, except that neither food nor food cups were left in either goal box. If *S* did not reach a goal box in 3 min., it was removed.

Part II: Extinction by nonfrustrating elimination of the primary drive.—The procedure for the 64 *S*s in Part II was essentially that described for Subexperiment II of Part I, i.e., ten preliminary massed trials were given to all *S*s on Day 1 in order to preclude differential elimination of slow learners in the massed and spaced conditions. For the first 32 *S*s, failure to reach either goal box within 3 min. was the criterion of nonresponse during extinction (as it was throughout Part I), but for the last 32 *S*s this criterion was changed to 1 min. This change was made because a preliminary analysis of the data for the first 32 *S*s showed little difference between massed and spaced extinction. It was thought that the many failures to run made the "massed" trials too "spaced." The 1-min. nonresponse criterion reduced such massed intertrial intervals from 195 sec. to 75 sec.

A 61-hr. food-satiation period preceded the first extinction trial of all *S*s. The satiation period was assumed to be long enough for recovery from any depressive effects of sudden overeating. Onset of satiation began 1.5 to 12 hr. after the last acquisition trial, being scheduled to allow individual *S*s to be run about the same time each day.

During satiation, fresh wet mash and dry food were kept in the cages. The wet mash was changed approximately every 12 hr., the last change coming 1.5 hr. before the first extinction trial. Each *S* was taken to the experimental room 15 min. before extinction began, and placed in a cage containing wet mash and dry food. The wet mash was changed every hour throughout extinction. A wet mash pellet was kept in each food cup of the maze. Uneaten pellets were replaced with fresh ones every 15 min. By having food in both food cups, the extinction procedure was rendered operationally "pure," i.e., it was extinction by removal of primary drive, not extinction by omission of a primary rewarding agent.

Treatment of data

The extinction data of the two subexperiments of Part I were pooled since an analysis of variance revealed no significant differences between them. The data involving the two criteria of nonresponse in Part II were pooled for the same reason. Prior to pooling in the latter case, the data of all 64 *S*s were summarized in terms of the 1-min. criterion. Thus, when any of the first 32 *S*s took longer than 1 min. to reach the goal box, that trial was treated as a failure to run.

All available time scores—time to leave the starting box, time to reach the choice point, total time, time spent in the stem of the maze, and time spent at the choice point—followed the same pattern in the various groups of Part I. Accordingly, only the total time required to reach either goal box, the most reliable measure of running, will be presented.

The response times for each *S* were used to obtain a frequency score. For each part of the research it was the number of trials on which *S*'s running time was below the over-all median running time for all trials for all *S*s (1200 trials in Part I, 1920 trials in Part II). This score was chosen not only because of the skewness of the raw time data, but because in Part II, *S*'s failures to run made both mean times and median times indeterminate for individual *S*s. The over-all median was selected as the cut-off point because it locates the mean of the scores close to the center of their distribution and because it minimizes the correlation between mean and variance. A third reason for selecting this measure was its similarity to the one used by Sheffield (19).

Results

Massed vs. spaced extinction

Extinction by frustrating omission of food.—In Fig. 2 is shown graphically the course of frustrating extinction as measured by running time and correct runs. The mean percentage of running times faster than the over-all median of 20.05 sec. is plotted on successive groups of ten trials. The mean percentage of correct runs (i.e., runs to the previously rewarding goal box) is plotted similarly.

The measures of extinction for running time and correct runs show differences in opposite directions. For

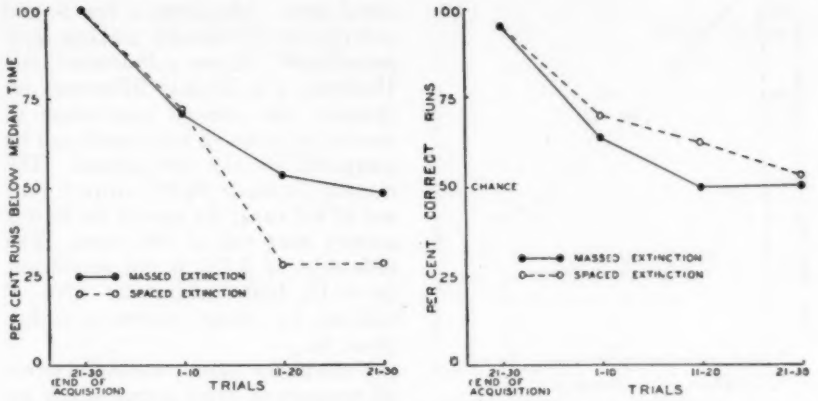

FIG. 2. The course of frustrating extinction in terms of runs to either goal box which were below the over-all median of 20.05 sec. and in terms of correct runs. Number of trials is the base for computing the percentages.

running time, the *massed* Ss were superior to the spaced Ss; for correct runs, *spaced* Ss were superior. The mean differences for both scores (4.4 and 2.1, respectively) are highly significant (p's $< .01$).[4]

Extinction by nonfrustrating removal of primary hunger drive.—In Fig. 3 is shown the course of nonfrustrating extinction in terms of running time scores, for both the massed and spaced extinction groups. Percentage of runs below the over-all median of 56.35 sec. is plotted on successive groups of ten trials. It is apparent from the figure that the two groups did not differ noticeably in rate of extinction.[5] For massed extinction the mean number of runs below the median was 15.1, for spaced extinction, 14.9, giving a difference well below the standard error of the difference which was 2.82.

It can be seen in Fig. 4 that there were many failures to run within the 1-min. nonresponse criterion used with satiated Ss. Whereas Ss entered a goal box over 80% of the time during the first two trials of extinction, they did so less than 40% of the time during the last ten trials. Therefore, Fig. 4 shows percentage of correct runs with total runs made instead of total trials as the base for computing the percentages.[6] Both groups approached a chance level of performance quickly, and there is a difference in favor of the spaced group holding up better than the massed. However, a statistical evaluation of this

[4] The p-values are for one tail of the distribution, unless two tails are specified. Statistical analyses are based on data from all 30 extinction trials.

[5] During the last few trials of acquisition, the massed extinction Ss ran significantly faster than the spaced extinction Ss. This might have reflected E's fatigue since the scheduling of training called for a more arduous work and sleep schedule on days before spaced extinction than on days before massed extinction. It is believed, however, that the difference in acquisition could have only slight effects on extinction because the slopes of the two curves are identical. There was no noticeable difference between the two groups in number of correct responses made during acquisition.

[6] In Part I, one S in the spaced-massed group and one S in the spaced-spaced group once failed to reach a goal box within the 3-min. nonresponse criterion.

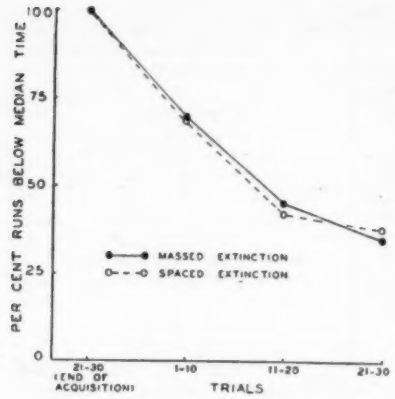

FIG. 3. The course of nonfrustrating extinction in terms of runs to either goal box which were below the over-all median of 56.35 sec. Number of trials is the base for computing the percentages.

difference requires a valid measure for individual *S*s. The percentage of runs correct cannot be used, for each *S*'s percentage score would be based on a different number of runs. Also, for several *S*s, the percentage would be quite meaningless; for example, having 1/1 equal 100 and 0/1 equal zero. Moreover, a few *S*s did not run at all, thereby making their percentage scores indeterminate. However, if individual differences are ignored, the over-all percentage of correct runs out of runs made can be compared for the two groups. The massed *S*s made 56.0% correct runs out of 495 runs; the spaced *S*s, 59.3% correct runs out of 494 runs. The difference of 3.3% is not significant ($p = .35$, both tails) even with N inflated by using responses rather than *S*s.

Perhaps the most meaningful over-all measure of habit is one which reflects both runs made and entrances into the previously rewarding goal box, i.e., the number of correct runs made. A statistical analysis of the difference in terms of this "composite" measure proved that the difference was small and not significant ($p = .77$, both tails).

The generalization decrement factor

Extinction by frustrating omission of food.—In Fig. 5 is shown the extinction curves of the four groups of Part

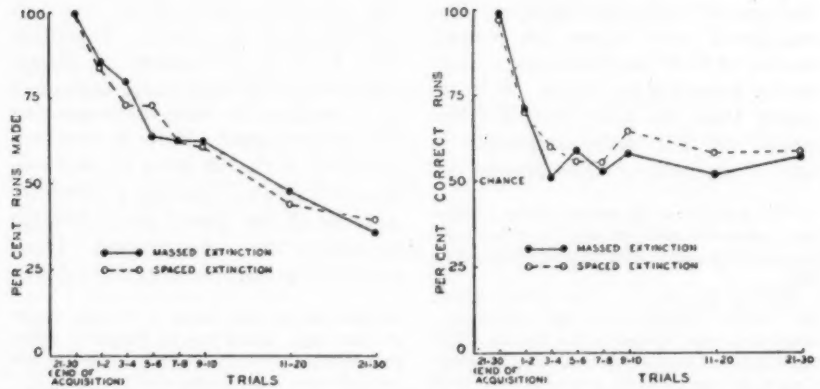

FIG. 4. The course of nonfrustrating extinction in terms of runs to either goal box within the time limit of 1 min., and in terms of correct runs. In the former, number of trials is the base for computing the percentages; in the latter, number of *runs made*.

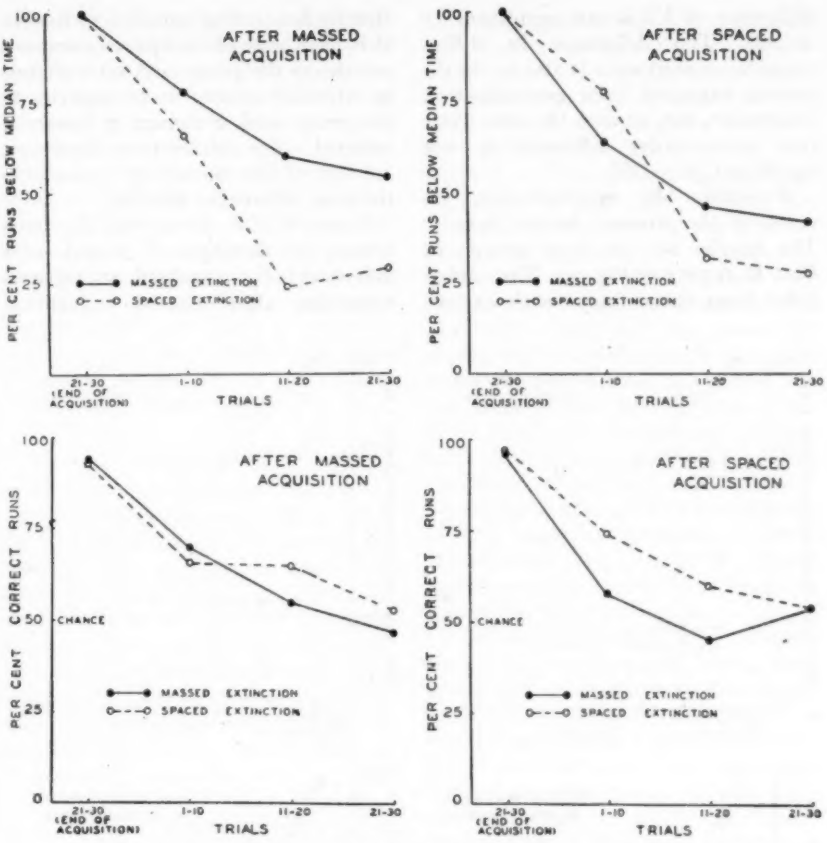

FIG. 5. The course of extinction under frustrating conditions for the massed and spaced groups, as a function of the distribution of acquisition trials. The upper graphs plot the runs to either goal box which were below the over-all median of 20.05 sec.; the lower graphs, correct runs. Number of trials is the base for computing the percentages.

I, for both running time and correct runs. With respect to running time, the massed-massed group is clearly superior to the massed-spaced group and the mean difference of 7.1 is highly significant ($p < .01$). However, the spaced-massed group is only slightly superior to the spaced-spaced group and the mean difference of 3.1 is not significant ($p = .15$). The difference in mean differences, though in the direction expected from generalization decrement due to inter-trial interval change, is not significant ($p = .15$).

With respect to correct runs, Fig. 5 indicates that just the opposite relationships hold. The spaced-spaced group is clearly superior to the spaced-massed group and the mean difference of 3.0 is highly significant ($p < .01$); while the massed-spaced group is only slightly superior to the massed-massed group and the mean

difference of 1.2 is not significant ($p = .12$). The difference in differences for correct runs is also in the direction expected from generalization decrement, but, as with the time data, this second-order difference is not significant ($p = .10$).

Extinction by nonfrustrating removal of the primary hunger drive.— The graphs for the four groups in Part II appear in Fig. 6. The curves differ from those found with extinction by frustrating omission of food in that each massed vs. spaced comparison shows the group without a change in intertrial interval to be superior to the group with a change in intertrial interval. For neither score, however, are any of the massed vs. spaced extinction differences reliable.

Figure 6 also shows that for both scores, the direction of second-order differences (i.e., massed vs. spaced extinction after massed acquisition

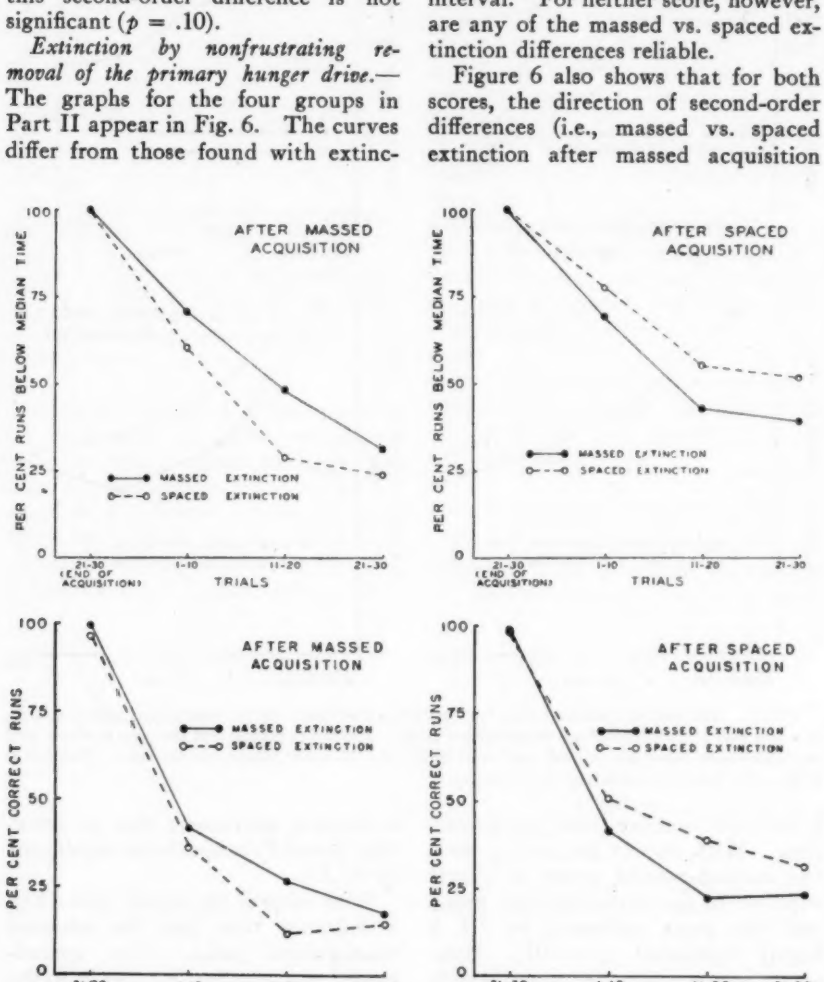

FIG. 6. The course of massed and spaced extinction under nonfrustrating conditions, as a function of the distribution of acquisition trials. The upper graphs plot the runs to either goal box which were below the over-all median of 56.35 sec.; the lower graphs, correct runs. Number of trials is the base for computing the percentages.

compared with massed vs. spaced extinction after spaced acquisition) is in the direction expected from generalization decrement. The reliability of the second-order difference for running time is about the same as that found in Part I. The independent p-values (.15 and .11) may be combined (7, pp. 46–47), and the over-all reliability of the differences expected from the generalization factor tested. When this is done, the combined p-value is .07.

For correct runs in Part II, however, the difference in differences achieves significance ($p = .05$).

Discussion

The running time data of Part I (frustrating extinction) confirm Sheffield's (19) finding that massed extinction trials produce less decrement than spaced extinction trials in terms of a vigor measure of performance. However, the equally significant difference in the opposite direction with correct runs as the measure indicates that the generality of the Sheffield finding is limited (*a*) to extinction in a restrictive situation (i.e., a straight alley situation), and (*b*) to a relatively nonspecific, and presumably a drive, measure of performance in the less restrictive situation (i.e., vigor of running to either goal box in a T-maze).

Another significant aspect of the present findings is the absence of a difference between massed and spaced extinction trials during extinction by omission of the primary hunger drive. Such extinction is presumably nonfrustrating. Therefore, if spacing per se of extinction trials results in greater resistance to extinction than massing of extinction trials, the superiority of spacing should have been revealed. The results provide no evidence for such superiority.

Finding no noticeable difference with nonfrustrating extinction operations suggests a frustration-produced drive interpretation of both the running time and the correct response data obtained in Part I, where extinction was by frustrating omission of food. As indicated earlier, we assume first that more of the frustration-produced drive (or excitement) was present at the start of each trial for massed *S*s than for spaced *S*s. It follows, then, that the massed *S*s should have reached either goal box sooner than the spaced *S*s, as they did. Assuming further that the greater the drive state and concomitant excited activity, the greater should be the likelihood of incompatible responses becoming prepotent over the specific turning response acquired, then it follows that the massed *S*s should have made less correct runs than the spaced *S*s, as they did.

Sheffield's "negative" finding, therefore, is explainable, as she suggested and as borne out by the present research, in terms of the effects of frustration-produced drive in a situation where behavior was essentially limited to locomotion in a straight alley. Sheffield's study, in which rats ran more rapidly with massed than with spaced extinction trials, and Rohrer's (17, 18) studies in which highly motivated, well-trained rats pressed a bar more frequently with spaced than with massed extinction trials, provide a contrast of results like that found with running time and correct response measures in Part I of the present research—and, apparently, for the same reasons, theoretically.

The failure to find a difference in Part II of the present research bears on several interpretations of the basic nature of the extinction process. First, the no-difference result is not in

accord with the expectation from Pavlov's (12) internal inhibition theory, Hull's (7) reactive inhibition theory, the fatigue aspect of Miller and Dollard's (10) theory, and Mowrer and Jones's (11) fatigue theory. Each of these theories postulates that response decrement during extinction is a direct function of some inhibitory factor (resulting from nonreinforced stimulus presentations for Pavlov or from responding for the others), and that this factor dissipates with time. Thus, with extinction not "contaminated" by frustration effects massed trials should have produced more decrement than spaced. This was not so. However, the no-difference result can be regarded as support for the Guthrie-Wendt (3, 21) interference-by-new-learning extinction theory by assuming that elimination of frustration due to nonreinforcment removed the major stimulus differences between the massed and spaced conditions. If this assumption is reasonable, the Guthrie-Wendt theory predicts no difference, as was found. It would appear that the theoretical import of the no-difference finding is such that its generality should be tested with other types of nonfrustrating extinction, e.g., the extinction of an escape habit by omitting shock.

The relevance of the present findings to drive-reduction extinction theories based either on conflict (10) or frustration (18) reduction is unclear without further research. For example, in Part I, it could be argued that massed *Ss* avoided the formerly rewarding goal box more often than spaced *Ss* because of the greater amount of frustration-produced drive being reduced by such avoidance. However, this argument would also predict more rapid extinction with massed trials in a straight alley, which Sheffield showed was not the case. Perhaps, the motivating effects of frustration-produced drive and the reinforcement effects of its reduction interact in some complex manner.

With respect to the factor of generalization decrement due to intertrial interval change, it must be admitted that conclusive statistical proof was not found, even though, in one instance—frequency of runs correct in Part II—the difference just attained significance. However, the pattern of results (Fig. 5 and 6) makes a fairly convincing case for generalization. The relative magnitudes of all the massed vs. spaced differences in terms of both scores are those which would be expected if generalization decrement had a small but real effect—too small to counteract the effect of frustration-produced drive (Part I), but appearing in terms of direction of massed vs. spaced differences in Part II where frustration was presumably absent. At the very least, the results suggest that the possible effects of such generalization decrement should be balanced if valid conclusions are to be drawn about the effects of distribution per se of extinction trials.

A number of previous investigations did not balance for generalization decrement and obtained results in line with its expected effect. Two such studies (13, 14) compared massed-massed with massed-spaced groups and obtained trends toward faster extinction with spaced extinction trials. One study (1) compared spaced-massed with spaced-spaced groups, and obtained faster extinction with massed extinction trials. And, as Reynolds (16) has noted, most of the Russian experiments have probably compared spaced-massed with spaced-spaced groups.

Actually, all the experimental evi-

dence[7] thus far reported on rate of extinction as a function of the distribution of extinction trials is, with one possible exception,[8] in accord with the expected effects of either the frustration-produced drive factor, the generalization decrement factor, or both factors, depending on which of these effects could be assumed to be involved. It would appear that the best way to summarize the present findings, as well as those of previous investigators, is to say: If spacing per se of extinction trials produces less rapid extinction than massing of extinction trials, the difference between the two conditions of extinction is either small, or specific to particular kinds of responses.

A difficult theoretical problem raised by this conclusion is how the findings are to be interpreted in the light of the well-known phenomenon of spontaneous recovery of an extinguished habit. At the very least, the present findings suggest the necessity for a re-examination of the factors determining spontaneous recovery.

Summary

The present research investigated rate of extinction as a function of the distribution of extinction trials. Massed and spaced extinction of a food-reinforced T-maze position habit in rats were studied. Half of each extinction group was trained with massed trials (15-sec. intertrial interval); half with spaced trials (one trial every 15 min.). In Part I of the research ($N = 40$) extinction was by frustrating omission of food; in Part II ($N = 64$), extinction was by nonfrustrating removal of primary hunger drive.

In Part I, extinction was more rapid with *spaced* extinction trials according to running time to either goal box (a vigor, and presumably a drive, measure of performance), but more rapid with *massed* extinction trials according to runs to the previously rewarding goal box (a measure of the specific habit acquired); in Part II, massed extinction did not differ from spaced in terms of either score.

These data and the over-all pattern of massed-spaced differences broken down according to distribution of acquisition trials conform to expectations based on the factor of frustration-produced drive and the factor of generalization decrement due to shifting from one intertrial interval during acquisition to another during extinction. In this respect the data are comparable, in general, to previous findings, but not in line with conclusions from extinction theories which assume that response decrement during extinction is a direct function of internal or response-produced inhibition (or fatigue) which dissipates with time.

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[7] For an excellent critical analysis of studies (2, 4, 12) which because of methodological shortcomings are not included here, see Sheffield's (19) recent article.

[8] In a study of the conditioned eyelid response (nonfrustrating extinction according to the present interpretation), Reynolds (16) found that after massed training, the massed and spaced extinction groups were about the same, possibly indicating that the spaced extinction group held up despite generalization decrement. Reynolds interprets this result in terms of a somewhat involved "stimulus intensity" generalization decrement theory.

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WORK AND REST FACTORS IN EYELID CONDITIONING[1]

SIMON VANDERMEER AND ABRAM AMSEL

Tulane University

Evidence from experimental studies indicates, in general, that repeated elicitation of a conditioned response with a relatively short intertrial interval leads to a reduction in strength of that response even when it is consistently reinforced. Pavlov (6) reported this finding and since then it has appeared in other studies of classical eyelid conditioning (2, 3).

Studies of the effect of massing and distribution of trials in eyelid conditioning have yielded more of the same kind of evidence. Humphreys (5) obtained a higher level of conditioning when he gave *Ss* 48 trials in two separate sessions rather than 48 trials in one session; but when one group of *Ss* was given 96 trials and another was given only 48 in the same amount of time, conditioning was equal for both. Calvin (1) conditioned eyelid flexion in three groups of *Ss*, the rate of presentation of double stimulation, regularly spaced, being 18, 9, and 3 trials per minute for the different groups. Throughout 50 trials, the frequency of conditioned response for blocks of ten trials did not increase more than 12% over the first ten trial response level for *Ss* having 9 or 18 trials per minute. For the 3 trials per minute group the frequency had increased more than 30% by the end of 50 trials.

All of these studies appear to indicate that some kind of labile inhibitory factor is operating to diminish response strength and that this diminution is a function of (*a*) rate of responding and (*b*) total work session activity. One of the most explicit attempts to integrate these response decrement phenomena into behavior theory has been made within the Hullian system (4). Hull has employed a concept *reactive inhibition* which he defines as a negative drive state resulting from responding. This state is postulated as having no relationship to goal events and develops whether responses are reinforced or not. The strength of reactive inhibition at any point in practice is, therefore, related to the number of previous reactions of a particular sort, the amount of work involved in each reaction, and the degree of spacing of the completed trials.

The present research was an attempt to provide evidence as to the manner in which reactive inhibition develops during a series of reinforced conditioned eyelid responses. The general experimental plan was (*a*) to vary the rate of stimulation in a series of conditioning trials given in two sessions with a 24-hr. interval between and (*b*) to vary the time at which a rest period was introduced during the second session when strength of conditioning was presumably at a maximum.

Experimental Procedure

Apparatus.—A 30-revolution-per-minute synchronous motor equipped with a rotor and contacts was used with a two-relay system to administer the stimuli and activate the time markers. Recording of time intervals and eyelid reactions was made by inkwriting equipment on blank white roll paper which was automatically wound on a kymograph drum. A double-pole knife switch controlled the starting of the

[1] Thanks are due to Drs. Robert B. Ammons and George Passey who offered important suggestions in the initial stages of designing the experiment and in setting up the apparatus.

kymograph and the motor activating the relays for presentation of the stimuli. A 50-in. nylon thread was attached at one end to *S*'s right eyelid immediately above the eyelashes with flexible collodion, then passed over two pulleys and attached at the other end to a heart lever and capillary pen. This was used to transmit and record the eyelid reactions. Slack in the thread was taken up by adjustment of the pulleys over which the thread passed to the heart lever. Equal tension was judged by reference to the return spring and stop for the lever and pen.

A change in intensity of a 15-w. light bulb behind a 4-in. square white glass screen 36 in. in front of *S* served as the CS, the light changing from .06 to .22 apparent ft.-candles as measured by the Macbeth illuminometer. The illumination in the center of the room (general illumination) was less than .02 apparent ft.-candles. The duration of the CS was 450 msec.

The UCS was a puff of air to the lower right eyelid of several milliseconds duration and it overlapped the terminal part of the CS. The source of air was a tank of compressed breathing air equipped with a double gauge showing both tank and line pressure. An air hose ran from the line gauge to a solenoid valve which was controlled by one of the relay systems. From the other end of the solenoid valve an air hose fitted with a nozzle led to the chair in which *S* was seated, and was adjustable so that the distance from the nozzle opening to the lower eyelid could be kept constant for all *S*s. The air nozzle was adjusted so that it was 2 in. from the outside lower corner of *S*'s right eye. The air was set at a line pressure of 15 pounds per square inch for all *S*s.

Subjects and design.—The *S*s were 64 college men enrolled in undergraduate psychology courses. They were assigned randomly to one of four subgroups in each of two main groups. One main group was conditioned at the rate of 9 trials per minute (3 to 11 sec. between trials, average 9 trials per minute). The other main group was conditioned at the rate of 3 per minute (15 to 25 sec. between trials, average 3 trials per minute). On the first day of training, every *S* was given 50 trials at the appropriate rate for the main group to which he had been assigned. These trials on the first day were given in order to establish a fairly stable level of conditioning, so that response acquisition would not obscure or complicate the effects of the work and rest factors being investigated in the experiment on the second day of conditioning.

On the second day Group 9 (average 9 trials per minute) and Group 3 (average 3 trials per minute) were each split into four subgroups of eight *S*s each. Every *S* received 80 trials on the second day. The *S*s in one subgroup of each main group had a 10-min. rest after the first 15 trials (hereafter designated as Group 9–15 and Group 3–15); another subgroup in each main group had a 10-min. rest after the first 30 trials (Group 9–30 and Group 3–30); another after 45 trials (Group 9–45 and Group 3–45); and the final subgroups had a 10 min. rest after 60 trials (Groups 9–60 and 3–60). The design is summarized in Table 1.

On Day 1 each *S* was given three spaced stimulations of light alone in order to gain an approximate index of reflex sensitivity to the change in brightness, and then given three spaced stimulations of air alone with the words "Be prepared for a puff of air to the eye" immediately preceding the first air stimulus. After the three air stimulations, *S* was again given three trials with light alone, in order to determine if there had been any sensitization effect by the air stimuli. Several *S*s were terminated at this stage for showing sensitization effects. One minute after the last single stimulation *S* was given 50 paired presentations of light and air in accordance with the conditions of the main group to which he had been assigned.

On Day 2 80 paired stimulations were given to each *S*, with rest at the appropriate interval for the subgroup to which he belonged. All *S*s named colors appearing in a memory drum during the rest period. The *S* was released from the chin rest and bite-board during the rest period, and the tension on the thread was relaxed although the thread remained attached to the eyelid.

Results and Discussion

In order to be classified as a CR, it was required that the response satisfy two criteria: (*a*) it must have appeared not earlier than 180 msec. after the onset of the CS, and (*b*) a closure or secondary closure must have begun before the onset of the UCS. Secondary closure is defined as closure after opening or partial opening of the eyelid subsequent to an incidental response, voluntary response, or a reflex to light. Frequency of anticipatory CR was used as the criterion measure.

The frequency (per cent) of anticipatory responses during the first day's session for the 32 *S*s of each

main group is shown in Fig. 1. The data are given in blocks of five trials. It seems clear that the two groups follow essentially the same course of conditioning on the first day.

The Day 2 curve for each group, also shown in Fig. 1, is the per cent frequency of anticipatory responses for the 80 trials of the second session. These curves show the data from all subgroups without regard to the 10-min. rest intervals. Frequency of response is somewhat variable throughout the 80 trials. Group 9, the relatively massed group, appears to show superior performance over a majority of trials with the exception of the first 15 trials after the 24-hr. interval between sessions. This segment of the curve (first 15 trials) is, of course, uninfluenced by the 10-min. rest periods which occurred at the various later stages for the subgroups throughout the remaining 65 trials. The 10-min. rests could, perhaps, account for some of the variabil-

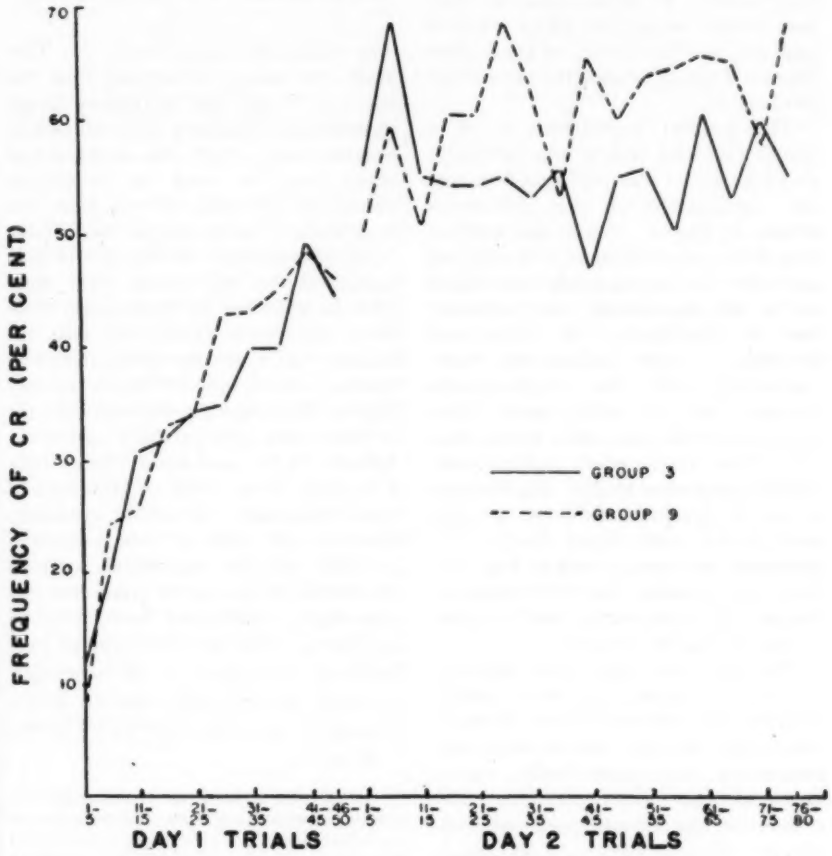

FIG. 1. Conditioning curves for Groups 9 and 3. $N = 32$ in each group.

ity in the Day 2 curves. Since, however, only eight *Ss*, or one-quarter the number of *Ss* in each curve, had rest introduced at any one of the stages, it seems unlikely that this rest effect would affect the shape of the curve profoundly.

Figure 2 shows a plot of the postrest increments on Day 2 against trial on which rest was introduced. Each point in Fig. 2 represents the percentage difference between CR's on the ten trials immediately preceding rest and the ten trials subsequent to the rest period. It seems apparent that rest results in greater gains when it appears later in a series of trials than when it appears soon after the session has begun.

The present experiment is of a simple factorial design, and an analysis of variance was performed to test the significance of the differences shown in Fig. 2. Since the interaction between position of rest interval and distribution of trials was found to be not significant, the variances due to distribution of trials and number of trials before rest were evaluated with the within-groups variance as the error term. The *F*-ratio *distribution/within groups* was .37. The ratio *trials before rest/within groups* was 18.21. Significance at the .01 level of confidence requires an *F* of 4.16 with 3 and 56 *df*. The difference between curves in Fig. 2 is, then, not reliable; the differences between the subgroups, within each curve, are highly reliable.

The per cent gain was approximately the same, in each group, whether rest followed 45 or 60 trials, indicating, perhaps, that a maximum temporary decrement builds up in about 45 trials in this situation. The gain after the 10-min. rest for Subgroup 3–60 was 14%, a gain almost equal to that made over the 24-hr.

FIG. 2. Mean gain of conditioned responses after 10-min. rest. $N = 8$ in each subgroup.

interval by the main Group 3. This might be taken to suggest that the majority of the rest increment occurs immediately following the cessation of practice, i.e., that the incremental effect may be near its maximum value after 10 min. of rest from the experimental stimulating conditions.

An examination of the results presented above will reveal that they seem to conform to expectation from work inhibition theory in two instances: (*a*) a greater postrest rise in level of conditioning on the second day follows the longer prerest sessions; (*b*) in both main groups there appears a definite 24-hr. increment—from Day 1 to Day 2—in level of conditioned eyelid response. Generally speaking, however, the kind of result obtained in other studies employing various conditions of spacing of trials was not apparently duplicated here, i.e., no significant difference in level of performance was observed on either day between the relatively massed group (Group 9) and the distributed group (Group 3).

Actually, the apparent disagreement between this latter aspect of our results and those of other investigators may not be as great as it seems at first. Spence and Norris (7) found no significant difference between conditioning curves where the

conditions of spacing of trials were 2, 4, and 7 per minute. They do, in fact, show an inversion of expected results with their 2 and 4 per minute conditions. A significant difference is shown between the curves for the 7 per minute and the 1 per 1½ minute conditions, but not between the 1 per 1½ minute condition and either of the other conditions. They indicate that except for the group with most widely distributed trials, all of the conditioning trials were given in a single session.

In Calvin's (1) study the 3 and 9 per minute groups showed the expected difference in level of conditioning, but there was an inversion in position of the 9 and 18 per minute curves. Conditioning was better with 18 trials per minute than it was with 9 per minute. Again, all trials for all groups were given in a single session. If Calvin had run only the 9 and 18 per minute conditions, his findings would have been essentially like ours, i.e., no difference in level of conditioning with two different degrees of spacing of trials. Humphreys' (5) results, also, can be interpreted as showing the importance of length of session in eyelid conditioning as easily as they can be interpreted in terms of massing of trials.

There is, then, the suggestion that although degree of distribution of trials is a variable in eyelid conditioning, other factors may obliterate the effect of this variable when the conditions of distribution are not sufficiently different. The equality of the two main groups for level of conditioned performance reached in the present investigation may actually have resulted from the presence of two sources of inhibition, one acting most strongly in each group. In Group 9 the rate of stimulation was high, the time in session short; in Group 3 the rate of stimulation was lower, the time in session longer.

Keeping in mind the sustained nature of the activity of the eyelid, especially as it is sensitized by the conditioning situation, and that work has been suggested as an inhibitor of the conditioned response, then the longer the trial session the more the accumulated work *in the situation*, regardless of what *E* thinks is the massing of trials. It may be, then, that this "work" is a function not only of rate of elicitation of the response, but also of time in session, especially in something like eyelid conditioning. There may also be a decrease in anxiety motivation with increased time in session, especially in human *Ss*. Taylor (8) has shown that the level of eyelid conditioning is clearly a function of degree of anxiety. This factor might also have been operating, in the present situation, against the facilitating effect of distributed practice.

From the considerations presented above, it appears that the eyelid reaction, particularly, may not be a suitable response with which to test theories of work inhibition, where work is defined by the degree of spacing of trials.

Summary

Conditioning of the eyelid was investigated under a plan in which the rate of stimulation was varied as well as the time at which rest was introduced in a series of trials. In order that incremental factors inherent in acquisition might not obscure decrement attributable to reactive inhibition, all *Ss* had one day's session of 50 trials plus at least 15 trials on the second day before a 10-min. rest was introduced. This rest came after 15, 30, 45, or 60 trials, respectively, for four subgroups in each of two main groups given paired stimulations of light and air at an average rate of 9 per minute and 3 per minute.

The results indicated that there was no difference in performance between the 9 per minute and 3 per minute conditions. On the second day, a greater postrest rise in level of conditioning followed the greater number of prerest trials under both rates of stimulation. The relationship between postrest increment and

number of trials before rest was an increasing one, apparently reaching its maximum at about 45 trials. A 24-hr. rest increment—between Days 1 and 2—was present in both main groups.

The apparent discrepancies between these results and some former studies were discussed in terms of elapsed time per session, and the nature of the response system under investigation.

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SPEAKING AND LISTENING SIMULTANEOUSLY

D. E. BROADBENT[1]

Medical Research Council, Applied Psychology Research Unit, Cambridge, England

As communications systems grow in complexity, it becomes increasingly likely that individual human beings will find themselves at the meeting place of several systems, and be required to listen and speak on all of them.

There has been much research on the masking of speech by other speech and by noise (e.g., **3**). But comparatively little attention has been paid to another possibility, namely, that speech arriving over a number of loudspeakers, telephones, or direct voices may lose its effectiveness in controlling behavior even though it is not masked. One aspect of this problem is the relation of the behavioral mechanisms used in speaking to those required for listening, since both may be needed simultaneously by those who use several communications channels.

Consequently a general technique, which is to be applied later to questions involving the arrival of two or more incoming messages, was first employed on speaking and listening simultaneously.

PROCEDURE

The Task.—The *S* was required to look at pieces of paper carrying abstract patterns. These were made up by dividing the paper into five numbered sections and placing one of the following symbols in each: square, heart, diamond, circle, arrow, cross, or straight line. One section on each paper was left blank, and one carried two identical symbols (Fig. 1).

At any one time, only one paper was visible to *S*, and he received a series of questions about the pattern on this paper from the loudspeaker of a wire recorder (Wirek, Type A). The intensity of playback was kept at a mean of approximately 75 db. (This record is of only moderate fidelity, about equal to that of the better radio-telephone channels usually met with.) Each question could be answered "Yes" or "No," and was worded in accordance with a typical radio-telephone procedure: *S* had to put his reply into the same form. Thus a question might be "Hello S-1. This is G.D.O. Is there a heart on Position 1? Over." And the correct answer, "Hello G.D.O. This is S-1. Yes. Over." There were two possible sources of messages—"G.D.O." and "Turret"—and *S* had to make his reply to the correct one. In addition he was instructed to ignore any message which did not start with the call-sign S-1. Consequently he had five choices for any given message: not to answer at all, and to answer either "Yes," or "No," either to "G.D.O." or to "Turret." His answers were compared with a check list and his errors noted.

A normal run on the task consisted of 31 questions, the paper being changed after the sixteenth question (to which no answer should have been made). The remaining 15 questions on each paper consisted of 12 questions to be answered and 3 not to be answered. During training on the task each question was separated from the next by a short interval in which the answer could be made: the shortest this interval could be made without causing overlapping with the next question was 3 sec. Each *S* was given 30-min. training on the day before the experiment proper, and all those used in the main experiment were able to reach a very high level of efficiency in this time.

Occasional overlap.—On the day of the main experiment Group Ia (12 *S*s) received two runs

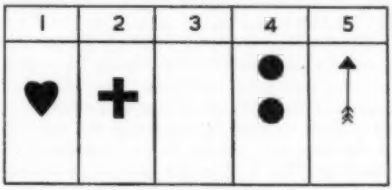

FIG. 1. A sample paper

[1] This work was done while the author was receiving a grant from the British Medical Research Council, and it is a pleasure to acknowledge the assistance given by the Royal Navy both in supplying *S*s and in providing the technical equipment.

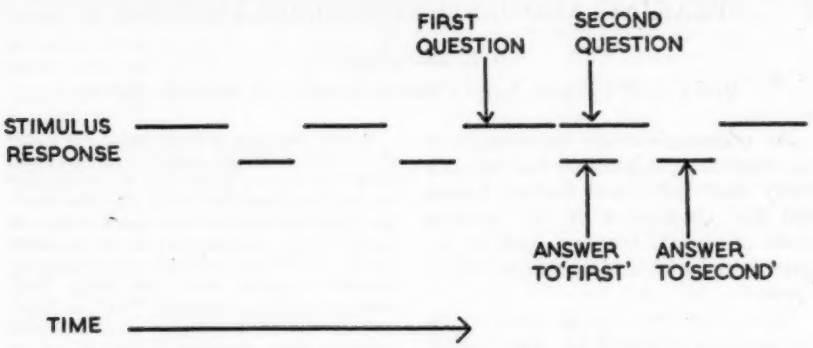

FIG. 2. Arrangement of questions and answers in occasional overlaps

of the type described above, but with the difference that ten of the questions in each run were followed immediately by the next question, so that the answer to one question had to be made while the next was being heard (see Fig. 2). In two cases out of the ten the second question was not to be answered, and in the remaining eight it was. In all cases the first question was to be answered. Two different recordings were used as a precaution against peculiarities of diction, but no difference appeared in the results and they have therefore been pooled. Group Ib (12 *Ss*) was treated identically to Group Ia except that the recordings used both in training and in the main experiment were phrased in a shorter form of radio-telephone procedure in which only the two call-signs, linked by the word "from," preceded each message. Thus the previous example could become: "S-1. from G.D.O. Is there a heart on Position 1? Over," and the answer, "G.D.O. from S-1. Yes. Over."

Continuous overlapping.—All of Group Ib and four *Ss* from Group Ia received a third run in which all the questions followed each other immediately, so that all the answers had to be given while listening to fresh questions. It is of interest that this third run had to be given after the other two runs and could not precede them, as *Ss* tended to despair of such a task if they had not had the experience of successfully carrying out the first two runs.

Short-answer conditions.—Group II (11 *Ss*) heard the same recordings as Group Ib, including the continuous overlaps. But they had previously been trained to use no procedure at all in their answers, merely giving the call-sign of the supposed sender of the question and "Yes" or "No." Thus the answer to our example would no longer be "G.D.O. from S-1. Yes. Over," but simply, "G.D.O. Yes."

General features.—When questions were not run together the time left for the answer was 4 sec., giving ample time under all conditions for an answer to be made. The main sentence of each message was always the same length, though the longer call-signs used in Group Ia increased the total length of each message in that group.

All *Ss* were warned at the beginning of the experimental session that questions might be run together, and were told to "carry on with what you are saying and at the same time get enough of the next question to answer that too" (see 4). They were also warned of the nature of the third run before beginning it.

The *Ss* were all naval ratings under the age of 25. Of Group I, 18 were Fleet Air Arm ratings and 6 of other trades, while of Group II, 6 were Fleet Air Arm ratings and 5 of other trades. Differences between individuals under the same conditions were throughout slight, compared with the differences between the conditions used. The task was designed to be superficially similar to an actual naval task.

RESULTS

Occasional overlapping.—Mistakes under each of the conditions are tabulated in Table 1. The heading "incorrect" implies that a response should have been made but was not or was made wrongly, while failures to remain silent after an irrelevant message appear under a separate heading. The term "firsts" is used for brevity to include any question which was immediately followed by

another question, and "seconds" for any question which immediately followed another.

If we consider the data on occasional overlaps for Group I, it is apparent from Table 1 that both "firsts" and "seconds" show more failures to produce correct responses than do the control questions: the differences shown are highly significant ($p < .01$) by a t test. Equally, S is more likely to answer a question which he should not, if it is heard while he is answering a previous question. This difference also is highly significant ($p < .01$). It should be noted, however, that, although there are slightly more failures on "firsts" when the "seconds" are not to be answered than there are on the control questions, this difference is small and quite insignificant ($p > .05$). The difference between this type of "first" and those in which the "second" does require an answer is significant ($.05 > p > .01$).

The incidence of failures was greater in Group Ib which used the shorter form of radio-telephone procedure: using the correlation method of Whitfield (7), the difference on "firsts" is significant ($.05 > p > .01$) and that on "seconds" highly significant ($p < .01$).

It should be noted that Group Ib not only gave shorter and more concise answers than Group Ia but also received shorter questions: consequently these differences do not contradict the results reported below, for the short-answer condition, but rather reinforce the view that forcing into simultaneity essential items of incoming and outgoing information will lead to impairment. The "carrier" phrases in the less concise messages afford an opportunity for attention to be paid to another task.

The incidence of failures on "seconds" appeared to be related to the type of response made to the corresponding "firsts": if the "first" was completely omitted only 16% of mistakes were made on the succeeding "second," as opposed to 18% if the "first" was correctly answered. This difference was significant ($.05 > p > .01$). If the "first" was answered incorrectly, that is, if a complete but mistaken response was produced, failures on the corresponding "second" rose to 67%. This differ-

TABLE 1

PERCENTAGE OF MISTAKES UNDER VARIOUS CONDITIONS

Group	Occasional Overlaps						Continuous Overlaps		
	Control Questions with Clear Space Before and After		"Firsts"		"Seconds"				
	Incorrect	Answered when Silence Was Correct	Second to be Answered	Second to be Ignored	Incorrect	Answered when Silence Was Correct	All Questions	Question Not Overlapped	Answer Not Overlapped
Ia	1	2	6	0	7	8	29	17	45
Ib	3	6	21	10	32	25			
Total for I	2	4	13	5	19	17			
II	3	16	22	20	15	25	30	24	29

was only barely significant ($p = .05$) as the number of cases was small.

Continuous overlapping.—In the continuous run of questions the 16 *Ss* gave an over-all frequency of failures of 29%. As, however, 12 of these *Ss* used the short procedure and the over-all percentage lumps together questions in situations of different difficulties, this figure is not very revealing. More important is the fact that the end of each sequence of questions shows more errors than the beginning, as may be seen from the following analysis.

The run was divided into several sections by the questions to which the correct response was silence. Immediately after each such question would come a question which *S* could hear without having to talk simultaneously. (In only 11% of the possible cases was any answer made when the correct response was silence.) The very first question in the series could also be heard without speaking simultaneously. The difficulty of the first question and of those which follow the irrelevant questions should then be much the same as that of the "firsts" in the previous section: the question was not overlapped, but the answer was overlapped, by the next question. This is in fact the case. The frequency of errors on these questions was 17% while that on "firsts" of the same *Ss* was 16.5%.

By the same argument we might suppose that those questions at the end of each section of the continuous run, i.e., those which immediately precede questions to which the response is silence, and the last question, would show an incidence of failures corresponding to that on "seconds": it was shown earlier that an irrelevant question had the slightest of effects on an answer which it overlaps. This prediction is not justified however; the frequency of failures on these questions is 45%, while that of the same *Ss* on "seconds" is only 27%. This difference is significant ($.05 > p > .01$). It is not due to a greater effect of the irrelevant questions under these conditions: the frequency on the last question alone, which has a clear space after it, is 56%.

The best interpretation of this result seems to be, that when receiving a series of messages at this speed, *Ss* begin the series satisfactorily but become increasingly unable to answer the questions as the series of relevant questions continues. An irrelevant question provides a pause and enables *S* to re-start, but the same process of increasing impairment also begins again. This effect might be regarded as a consequence of the speed of the incoming information, which causes *S* to lag further behind until he breaks down (**5**), but it is not proved conclusively to be so.

Short-answer conditions (control group).—Group II may be compared with Group Ib since the latter answered at greater length the same questions. In the occasional overlaps, there was little difference between the two groups as regards failures on "firsts." However, for "seconds" there was a marked and highly significant difference, the shorter response resulting in less failures on "seconds."

In the continuously overlapping series, Group II showed the same pattern of performance as Group Ib. That is, those questions in the continuous run which were overlapped to the same extent as "firsts," received as many failures as did "firsts." Just as in Group I, however, those questions in the continuous run which were overlapped to the same extent

as "seconds" received significantly more failures than did "seconds."

Discussion

These experiments reveal two separable limitations on the effectiveness of speech communication. On the one hand, there is a "speed" factor in the situation: *the increase in rate of arrival of messages made possible by listening to more than one channel will cause a deterioration in the number of messages producing a correct answer.*

The evidence for this conclusion may be summarized as follows: (a) Failures on "firsts" are not affected by the change to the shortened answer technique, and are, therefore, due rather to the shorter time span of the perceptual situation than to the overlapping of perception and response. (b) A continuous series of relevant messages shows a pronounced deterioration at its end, which is abolished by an irrelevant message (i.e., is not a stable "fatigue" state), and which also survives shortening of response. (c) The pattern of performance in (b) resembles that shown by Vince (5) as a gradual falling behind of the response when the rate of stimulation exceeds the maximum possible rate of response.

On the other hand, the lower incidence of failures on "seconds" in Group II shows that *the saying of even a simple series of words interferes with the understanding of a fresh message*, because their task was the same as that of Group Ib as far as understanding the message and deciding between the alternative answers. In speech, unlike many laboratory tasks, we are dealing with a series of responses whose production in the correct order depends, after the beginning of each series, on factors internal to the organism. And it appears that this organization of a series of responses interferes in some way with the simultaneous reception of fresh stimuli.

The overlapping of stimulus and response in this situation resembles that appearing in experiments on "psychological refractory period," such as those of Vince (5); but in the latter a single response rather than a series is involved. On the other hand, a similar relation between the carrying out of a series of responses and the arrival of fresh stimulation is found in classical experiments on "distraction," such as those of Hovey (1) and Weber (6). In such work there are usually two differences from the present situation. First, the extra stimulus has no response attached to it by the instructions, but is supposed to attract attention by its intensity or intrinsic interest, e.g., mental arithmetic and the stating of the answer obtained may be accompanied by loud noise, or a humorous story. And secondly, the extra stimulus usually overlaps not only a series of responses but also the stimuli to those responses (in the case cited, the presentation of a fresh sum), and no distinction is made between these two situations. As far as overlapping with the response is concerned, the present results support the conclusion usually made that "distraction" depends on the importance of the extra stimulus for *S*: an irrelevant question interferes much less with an answer simultaneously made than does a relevant question.

The explanation of this interference arising from sequential response is uncertain. Simple acoustic masking of one voice by the other is unlikely: not only is a single voice an in efficient masker, but the intensity and timing of *S*'s voice are under his own control (3). One might suppose, by analogy with such work as that of Lee (2) on the control of speech at the syllabic level, that the "feedback" from each item in the response series is essential to the production of the next item and consequently that the interference reduces to competition in the perceptual mechanism. Alternatively, since we must admit some activity other than feedback to be operative in the selection of the next

response from the various possibilities, it may be that this activity is itself incompatible with perception. The former alternative, it must be noted, is not favored by the evidence for an increase in interference when the series of responses has been made incorrectly. For in this case it must be some effect of the wrongness of the first response which is interfering with perception, and such an effect would normally be classed as an extra response.

In general then, we may conclude that we cannot attend perfectly to both the speech of others and to our own, but whether this is because we are attending to what we have just said or to what we are just about to say remains uncertain.

Summary

To reproduce a situation in some modern communications systems, 24 *Ss* were required to give verbal answers to simple questions coming from a loudspeaker, and were sometimes presented with a fresh question while still answering a previous one. When occasional pairs of questions caused simultaneous speaking and listening both the interrupted response and the response to the interrupting question were significantly impaired, but the extent of the impairment was not very serious. Nearly 90% of the interrupted responses were correctly completed, and over 80% of the interrupting questions were sufficiently well understood to produce correct answers; as compared with 98% correct answers when neither question nor answer overlapped. When the answer to each question overlapped with the next question, a more serious impairment appeared. Only 70% of such questions were correctly answered, and the impairment seemed to grow worse as the sequence of questions continued.

In a control group, 11 *Ss* were trained to show understanding of the questions by a very short answer, and were then exposed to the same two conditions as the experimental group. In the second condition, and in the interrupted questions of the first condition, the degree of impairment was equal for the two groups. This suggests that the speed of presentation of the messages was an important factor independent of any overlap between speaking and listening. However, the control group gave significantly more correct answers to the interrupting questions of the first condition. We must therefore suppose some interference between speaking and listening as such, in addition to the perceptual difficulty of handling a rapid series of messages.

Further conclusions were that: (*a*) The interference with the interrupting questions varies with the correctness or otherwise of the response which is interrupted: a correct answer produces more interference with simultaneous listening than does a failure to answer at all, and there is some evidence that a mistaken answer produces more interference than a correct answer. (*b*) The use of a call-sign procedure which excludes "carrier" phrases such as "Hello" and "This is," while possibly reducing the time taken for each message, increases the interference between speaking and listening.

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TESTS OF TWO HYPOTHESES OF LATENT LEARNING[1]

JOHN P. SEWARD, WILLIAM E. DATEL, AND NISSIM LEVY[2]
University of California, Los Angeles

Studies of latent learning in a maze agree in finding a drop in errors even before food is introduced (**4**, **5**, p. 89). Fitting this fact into a Hullian framework, Meehl and MacCorquodale (**4**) propose the following hypothesis to explain latent learning: In the prereward trials the response of taking the correct path at a choice point builds up slightly greater habit strength than that of turning into a blind alley. On the critical trial, when a rat finds food in the goal box, a food drive is conditioned to maze cues. The next time the animal is put back in the maze this secondary drive is activated along with the primary hunger drive. When multiplied by the degrees of habit strength already existing it thus increases the difference in reaction potential between the two responses. The result is an abrupt fall in the number of errors.

A simple numerical example may make the point clear. Consider one choice point with a right turn leading to an empty goal box A and a left turn leading to a blind alley B. Suppose that after ten nonrewarded trials under primary drive $D_1 = 5$ a rat has formed a reaction potential of 20 units to the right and a reaction potential of 15 units to the left. In Hull's system, much simplified, these conditions imply right and left habits of four and three units, respectively. Thus:

$$\begin{aligned} E_R &= D_1 \times H_R = 5 \times 4 = 20, \\ E_L &= D_1 \times H_L = 5 \times 3 = 15. \end{aligned} \quad (1)$$

Assume that the finding of food in A produces a secondary drive D_2, also of five units. Its effect will be to multiply the existing reaction potentials by five:

$$\begin{aligned} E_R &= D_1 \times H_R \times D_2 \\ &= 5 \times 4 \times 5 = 100, \\ E_L &= D_1 \times H_L \times D_2 \\ &= 5 \times 3 \times 5 = 75. \end{aligned} \quad (2)$$

Thus the difference between them will be five times as great as before and the probability of an error markedly reduced. Notice that according to this theory it makes no difference *where* in the maze the rat finds food, since D_2 affects all existing habits indiscriminately. Meehl and MacCorquodale state: "On the present hypothesis, it should make little difference whether food was found in the goal-box or not, *so long as a feeding occurs in the maze or maze-like situation*" (**4**, p. 178).

Our own hypothesis of latent learning (**5**, **6**), on the other hand, is essentially a place-learning theory. It hinges on the concept of the fractional antedating, or surrogate, response. During nonrewarded trials in the above example we assume that surrogate responses to A and B are associated with right and left cues, respectively, at the choice point, and that these responses account for the difference in reaction potential already noted. Thus, setting $H_R =$

[1] Aided by the Faculty Research Committee, University of California. Exp. III was reported at the meeting of the Western Psychological Association, San Jose, Calif., April 27, 1951.

[2] Now at the University of Missouri.

$H_L = 1$ (5, pp. 86, 92), we have:

$$\begin{aligned} E_R &= D_1 \times {}_rs_A \times H_R \\ &= 5 \times 4 \times 1 = 20, \\ E_L &= D_1 \times {}_rs_B \times H_L \\ &= 5 \times 3 \times 1 = 15. \end{aligned} \tag{3}$$

Finding food at A conditions a goal response to the cues at that point. By stimulus generalization the surrogate goal response (${}_rs_G$) will also be aroused by choice-point cues, but more strongly, due to ${}_rs_A$, by those on the right. If we assume that ${}_rs_G$ contributes five units of secondary drive on the right and three on the left, the next trial will give the following picture:

$$\begin{aligned} E_R &= D_1 \times {}_rs_A \times {}_rs_G \times H_R \\ &= 5 \times 4 \times 5 \times 1 = 100, \\ E_L &= D_1 \times {}_rs_B \times {}_r\check{s}_G \times H_L \\ &= 5 \times 3 \times 3 \times 1 = 45. \end{aligned} \tag{4}$$

Again we can predict a fall in errors. But notice what would happen in this case if, instead of finding food in A, the rat should find it in B. The stronger ${}_rs_G$ would now be aroused on the left, giving E values as follows:

$$\begin{aligned} E_R &= D_1 \times {}_rs_A \times {}_r\check{s}_G \times H_R \\ &= 5 \times 4 \times 3 \times 1 = 60, \\ E_L &= D_1 \times {}_rs_B \times {}_rs_G \times H_L \\ &= 5 \times 3 \times 5 \times 1 = 75. \end{aligned} \tag{5}$$

We are forced to predict that the rat will now be more likely to turn left than right, i.e., *against* his former preference.

Evidence that the food place is important may already be found in the experiments of Herb (2) and Seward (6). But we felt that the question called for a more direct attack. The three experiments reported below were designed to test these two hypotheses by comparing the effects of introducing food on the same side as the animal's previous preference and on the opposite side. The first experiment, a modification of Blodgett's (1) rectangular maze study, was exploratory but not without incidental interest. The second and third were elaborations of Seward's (6) previous experiment.

Experiment I

Method

A rectangular alley maze was used in which two boxes formed part of the path, a cubical wooden box (B) on one side and a cylindrical metal one (A) on the other. Twenty-one male hooded rats were assigned at random to three groups, one control and two experimental. After preliminary adaptation they were given one trial a day for 17 days followed by three trials a day for five days. During this training period all *S*s were allowed to proceed through one of the boxes and removed from the other; those of Group C found food and were permitted to eat in the box from which they were removed. For a given *S* the removal box was the same on all trials.

On Day 23 each *S* was put directly into the two boxes for 2 min. each; in one of them he found food. Thirty to 40 minutes later he was given a single trial in the maze. In Group C and one Group E the food box was the one from which he had been removed during training; in the third group it was the opposite. Box and order effects were counterbalanced within groups.

Results

From reinforcement theory we had predicted that on the postreward trial both experimental groups would choose the path leading to the box from which they had been removed during training. Place-learning theory predicted that they would go to the box in which they had just been fed. Actually, six out of the seven *S*s in each experimental group chose the path leading *away* from the removal box. On the assumption that prereward training led to a preference for the *longer* path a reinforcement theory would predict the results obtained. Observation of *S*s' behavior confirmed such an inter-

pretation, suggesting that being picked up was inhibiting rather than rewarding. But since results by this method might reflect the stereotyping effect of a mild fear, we decided to change the technique rather than to add enough cases to justify statistical tests of significance.

Experiment II

Method

Design.—In revising our method there were two objectives: (*a*) to avoid setting up any bias due to fear; (*b*) to include a direct test of the Meehl-MacCorquodale hypothesis by repeating their technique of feeding one group in a maze-like box outside the maze itself. For the first purpose we adopted a single-unit T-maze with distinct end boxes and gave equated treatment on both sides. For the second we added a third box and split the *S*s into three groups matched for position preference as shown in prereward training. One group (P) was fed in its preferred box, the second group (N) in its nonpreferred box, and the third group (C) in the control box. According to the Meehl-MacCorquodale hypothesis all three groups should now choose their preferred side, while our own prediction was that Groups P and N would choose differently.

Apparatus.—Figure 1 shows a floor plan of the maze. The end boxes were like boxes *A* and *B* of Exp. I in all respects except that they had only one door. The third box, *C*, was of the same width, height (5½ in.), and material (unpainted redwood) as the maze alley. Removable blocks on three sides of the choice point could be used to convert the maze into a straightaway leading directly into *C*. Transparent sliding doors, manually operated, were located at the entrances of all three end boxes and at the starting box and choice point. A strip of plain linoleum flooring across the choice point, extending equally into both arms, was replaced before every free trial to prevent tracking. The two partitions (*P*) were not added until Exp. III.

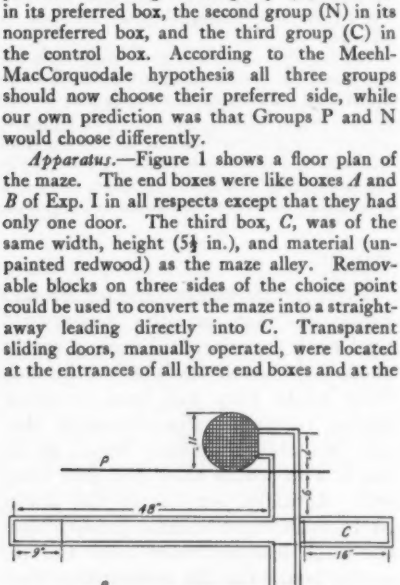

Fig. 1. Floor plan of maze used in Exp. II and III

Subjects.—Sixty-seven male albino rats, 3 to 4½ months old, were given prereward training; 60 were retained as a result of matching.

Adaptation procedure.—Animals were put on a feeding schedule of wet mash once a day and handled for five days. The next two days they were put several at a time in the straightaway to box *C* for 5 min. a day. For two more days they spent 5 min. in each of the three boxes, *A*, *B*, and *C*.

Prereward training.—With box *C* closed off, two trials a day in the maze were given for nine days. All trials were by the noncorrection method. The first trial was free; the second, 15 to 30 min. later, was forced in the opposite direction. Confinement in either end box was for 30 sec. Animals were returned directly to their living cages and fed for 30 min. after an interval of at least 45 min. from the second trial. Every third day before trials each *S* spent 2 min. in box *C* with the door closed.

On the basis of proportion of choices to one side in the nine free prereward trials, *S*s were matched in threes. Within each trio they were randomly assigned to Groups P, N, and C.

Reward day.—Each *S* was put directly into one of the three boxes for 2 min., where he found food (mash) for the first time. For Group P it was the box, *A* or *B*, on *S*'s preferred side as determined by his prereward choices; for Group N it was the nonpreferred box; for Group C it was box *C*. After an interval ranging from 14 to 32 min. (mean = 21) each *S* was given one free trial in the maze. A choice was recorded if the base of the tail passed the door on either side of the choice point.

Results

Of the 20 *S*s in each group, the number choosing their previously preferred side on the postreward trial was as follows: for Group P, 18; for Group N, 14; and for Group C, 15. Significance tests for correlated proportions were ruled out by small frequencies in the crucial cells, but the differences are obviously unreliable. Pooling the results we find that 47 out of 60, or 78%, chose according to preference. Moreover, a biserial correlation between number of left

choices in prereward training and right or left choice on the test trial gave $r_{bis} = .67 \pm .16$.

Discussion

If we accept position preference as a measure of differential habit strength, our results again appear at first glance to favor the reinforcement hypothesis. But to confirm a theory of latent learning we must first show that "latent learning" has occurred; i.e., that our results are due in part to the introduction of reward. To do so we must demonstrate a greater uniformity of choice after the experimental feeding than before it. For this purpose we made two comparisons:

1. The proportion of preferred-side choices on all prereward trials was 70%; on the test trial it was 78%. To test the significance of this difference we reasoned that on any one prereward trial 42 rats (70%) might be expected to follow preference. Dichotomizing the distribution between the 42 rats with the strongest preference and the 18 with the weakest, and applying McNemar's (3) test for correlated proportions, we obtained $\chi^2 = .76$, $p = .70$.

2. The percentage of preferred-side choices on the last prereward trial was found to be 75, almost as large as on the test trial. A biserial correlation between choice on Trial 9 and choices on Trials 1 to 8 gave $r_{bis} = .67 \pm .16$, identical with the figure already obtained for the postreward trial.

The only conclusion to be drawn from these findings is that the introduction of reward had no demonstrable effect on subsequent choice behavior. This result is surprising, since in a similar situation the senior author (6) found that over 80% of his rats chose the path leading to food. In that study hooded rats were used instead of albinos. But this cannot explain why reward had no apparent effect in the present experiment. We should expect drive conditioning in albinos, with inferior visual discrimination, to be more generalized than in hooded rats and thus more likely to favor the Meehl-MacCorquodale hypothesis, but not to be absent altogether. A more likely explanation of these negative findings is that, possibly owing to the use of forced trials, position responses were already too well established to be further enhanced by food.

Experiment III

Method

In the earlier study (6) feeding and choice were separated by only a few seconds. We therefore ran a third experiment embodying this condition. Two other distinguishing features were the use of hooded rats and the virtual elimination of forced trials. In general the method of Exp. II was followed except as noted below.

Apparatus.—Instead of converting the maze into a straightaway, runway adaptation was given in a second alley, like the maze stem, leading into box *C*. A second box was also substituted for box *C*, of the same dimensions but painted gray, with a linoleum floor like the strip at the choice point. During the rest of the experiment this box was in the position shown in Fig. 2. Two black partitions (*P*) were added to ensure concealment of the end boxes from the maze stem.

Subjects.—One hundred hooded rats were started, 89 males and 11 females, ranging in age from 67 to 138 days; 93 of them were assigned to three matched groups for testing.

Adaptation procedure.—From one to four days after being put on the feeding schedule, *S*s were given 10 min. a day for two days to explore in groups the straightaway to box *C*, followed by two individual runs a day for two to four days in the same apparatus. For the next three days they were allowed to explore the maze for 10 min. a day in small groups.

Prereward training.—This period was extended from nine to 12 days. Forced trials were discarded in favor of massed free trials until *S* had chosen each side at least once a day. In the vast majority of cases, owing to a strong tendency to alternate, two trials a day were enough. Owing to the same tendency, changing linoleum between trials was abandoned as unnecessary. End box doors were left open but *S* was kept in one side of the maze until he had entered the box, from which he was removed after 30 sec. After his trials each *S* spent 1 min. in one of the three boxes, *A*, *B*, or *C*, with the door closed, taking them one a day in rotation. At least 20 min. intervened between experimenting and feeding, a period spent in individual detention boxes.

Matching was based on three criteria in order of importance: (*a*) number of first choices to one side out of 12; (*b*) number of first choices to one side in the last five days; (*c*) total number of choices to one side including repetitions.

Reward day.—A thirteenth pair of trials was first run just as on the preceding days. A cup of mash was then placed outside each endbox to equalize odors. Each *S* was then given one of

several treatments followed immediately by one free trial in the maze. For Groups P and N the treatment consisted of being placed directly in one end box for 1 min., then transferred to the other for 1 min. Group P found food only in the preferred end box, Group N only in the non-preferred. Half the *S*s in each group, selected at random, found food in the first endbox and none in the second; for the other half the order was reversed. Group C was simply put in box *C* with food for 1 min., then given a free trial. If *S* failed to eat within 1 min., he was left in the box until eating "in earnest."

Results

Table 1 presents the data needed to test our two hypotheses. The deviation of postreward from mean prereward choices in terms of $SD_{est.}$ yielded the values of t (corrected for continuity) which are shown. The significance levels are computed for 11 *df*.

Meehl and MacCorquodale's (4) conditioned drive hypothesis implies, other things equal, that on the test trial there should be an increased selection of the side already preferred; this should hold true for all three groups, individually and combined, and no more for one than for another. None of these predictions is borne out by the data.

By the surrogate response hypothesis we should expect Groups P and N to diverge on the test; preference should be intensified for the former but reversed for the latter. Results were in the expected direction, but only the drop in Group N reached the 5% level of confidence. Another way of testing the same hypothesis is to compare the number of food-side test responses, for Groups P and N combined, with the number to be expected from prereward performance. Forty-five rats (72.6%) chose the food side, compared with a pretest mean of 31.6 (50.9%); with $SD_{est.} = 3.06$ and $t = 4.22$, the null hypothesis can be confidently rejected.[3]

Before testing group differences for significance it was necessary to determine whether the matching procedure had restricted sampling variation. Two biserial correlations were computed. The first, based on all groups, was between number of first choices to the left in 12 days of prereward training and first choice to right or left in the thirteenth pair of trials just before the test. The second was between 12 prereward choices and test choice for Group C alone. Values of r_{bis} were $+.02 \pm .13$ and $-.07 \pm .23$, respectively. In the absence of correlation between our matching criterion and performance on a single trial, we felt justified in assuming independent sampling variation for our groups.

Application of the χ^2 test for uncorrelated proportions gave the results shown in Table 2. Over-all differences among the three groups and the one between Groups P and N proved significant at the .01 and .001 levels.

TABLE 1

NUMBER OF PREFERRED-SIDE CHOICES, EXPERIMENT III

Group	*N*	Prereward Trials*		Postreward Test Trial	*t*	*p*
		Mean	$SD_{est.}$			
P	31	20.1	3.18	24	1.08	.30
N	31	19.5	3.40	10	2.65	.05
C	31	20.0	2.80	16	1.25	.30
Total	93	59.6	6.73	50	1.35	.20

* Statistics are for first choices on 12 trials for each *S*.

[3] As a supplement to the *t* tests reported here and in Table 1, we used χ^2 to test the independence of mean prereward and postreward frequencies of preferred-side as well as food-side choices. The formula for uncorrelated proportions was used. All conclusions were confirmed at comparable, though in some cases lower, levels of significance.

TABLE 2

SIGNIFICANCE OF DIFFERENCES BETWEEN GROUPS IN PREFERRED-SIDE TEST CHOICES, EXPERIMENT III

Groups	χ^2	*df*	*p*
P-N-C	12.81	2	.01
P-N	11.00	1	.001
P-C	3.45	1	.10
N-C	1.65	1	.20

Results of this experiment favor the surrogate-response theory of latent learning. To evaluate them properly, however, we must bring them together with those of Exp. I and II.

DISCUSSION

The above experiments were undertaken to decide between two hypotheses of latent learning; Meehl and MacCorquodale's drive-conditioning theory (**4**) and Seward's (**6**) surrogate-response theory. In Exp. I results on a few *S*s agreed with the former hypothesis; they were not followed up because the method introduced an avoiding reaction. In Exp. II findings again favored reinforcement theory but were rejected for lack of evidence that introducing reward had any effect. Experiment III supported Seward's theory. It may be objected, however, that this experiment, too, was unsatisfactory in that it failed to establish a reliable position preference before the test. The near-zero biserial correlations give no ground for assuming that prereward choices were more than a sampling distribution around a parameter of "no preference." Externalized drive could hardly enhance a habit differential that did not exist.

We cannot therefore reject one hypothesis in favor of the other. To establish either one seems to require rather special conditions. Much depends on the strength of previous position habits: if these are too strong both mechanisms may be obscured; if too weak, only place- and food-anticipations can be tested.

Actually, the two hypotheses are not incompatible. On further analysis the constructs involved may be found to refer to the same process. Meehl and MacCorquodale's secondary drive, for example, may be coordinated with our surrogate goal response; i.e., $D_2 = rs_G$. Substituting one for the other in equations (2) or (4) above, we find that the essential difference between the two hypotheses is the degree of generalization for which they provide. Both invoke the conditioning of a drive: for one it generalizes completely to the learning situation; for the other, the gradient may be steep or flat. In this respect the Meehl-MacCorquodale hypothesis becomes a special case of surrogate-response learning.

If a single hypothesis is to do the work of both it must be made more specific. We now see that rewarding the weaker of two habits will not necessarily reverse their selection. Whether it does so will depend on at least two conditions: (*a*) the steepness of the generalization gradient between the two habits; (*b*) the difference in habit strength already present. To illustrate the second point let us use the numerical example in the introduction and assign to rs_A and rs_B values of 5 and 2, respectively. Before the test we have:

$$\begin{aligned} E_R &= D_1 \times rs_A \times H_R \\ &= 5 \times 5 \times 1 = 25, \\ E_L &= D_1 \times rs_B \times H_L \\ &= 5 \times 2 \times 1 = 10. \end{aligned} \qquad (6)$$

After food was found in *B*, calculated

values would be as follows:

$$E_R = D_1 \times rs_A \times r\mathfrak{z}_G \times H_R = 5 \times 5 \times 3 \times 1 = 75,$$
$$E_L = D_1 \times rs_B \times rs_G \times H_L = 5 \times 2 \times 5 \times 1 = 50. \quad (7)$$

The difference $E_R - E_L$, instead of being reversed, is actually increased, a result which, taken by itself, would support the Meehl-MacCorquodale theory. This analysis bears out what the data suggest; that the only "crucial experiment" would be a systematic variation of the conditions determining relative response strength and stimulus generalization.

Summary

Meehl and MacCorquodale's drive-conditioning theory of latent learning differs from Seward's surrogate-response theory in stressing habit-strength difference before reward and in minimizing the role of place learning. Three experiments were done to compare the relative importance of position preference and food location in determining postreward performance.

Experiment I was modeled on Blodgett's rectangular maze study. Preliminary results favored the drive-conditioning theory but were complicated by avoidance behavior.

Experiment II made use of a single-unit T-maze with distinctive end boxes. Three groups of rats equated for prereward preference were fed in one of three boxes: the preferred end box, the nonpreferred end box, or an extra-maze box. When tested about 20 min. later they failed to demonstrate latent learning.

Experiment III was similar but shortened the time between feeding and testing to a few seconds. Test performance was significantly related to place of eating, as predicted by the surrogate-response hypothesis. Conditions were inadequate to test the opposing theory since reliable position preferences were not established.

These experiments led to a more precise specification of the relation between the two theories in terms of stimulus generalization.

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HE EFFECT OF SOME EARLY EXPERIENCES IN THE LATENT LEARNING OF ADULT RATS

RICHARD CHRISTIE

Research Center for Human Relations, New York University[1]

One of the more perplexing problems facing the learning theorist is the inconsistency of results reported in experiments which are apparently similar. This has been especially true in latent learning experiments where *S*s have demonstrated a marked predilection for displaying behavior appropriate to the theoretical bent of *E*.

One exception has been the consistent results reported by *E*s who have repeated the experiment by Spence and Lippitt (6). In the original experiment rats were trained in a Y-maze under a strong thirst drive. The *S*s were rewarded in the goal box containing water and had an equal number of unrewarding exposures to the irrelevant incentive (food) in the opposite goal box. After learning the location of the relevant reward, the motivation was shifted to hunger and the *S*s continued to the goal box containing water rather than to the one containing food.

The basic design of the Spence and Lippitt experiment has been replicated at least five times (2, 3, 4, 5, 7). These experiments all used strong irrelevant motivation, with asymmetry of reward, in a single choice point maze in which the drive dominant in the training period was rewarded.

All *E*s have found virtually unanimous choices to the previously rewarding side on the first test trial under reversed motivation. This is true whether the maze be a Y (3, 6), T (2, 4, 5) or diamond (7); whether thirst (3, 4, 5, 6, 7) or hunger (2, 4) was the motivation during training; whether the amount of reward during training varied from 20 sec. (4) to 3 min. (2) eating under hunger or from 5 sec. (6) to 30 sec. (3) drinking under thirst; whether the number of reinforced trials varied from 6 (5) to 40 (7); whether *S*s spent from 5 sec. (5) to 2 min. (2) in the goal box containing the irrelevant incentive; or whether differential cues were used to enable *S*s to discriminate more readily between the two alleys (3, 4, 5, 7).

In short, we have here a highly stable type of performance which is seemingly resistant to minor variations in procedure or maze design as long as the basic design is followed. The author was therefore impressed when some *S*s subjected to early deprivation of food and water (designed to give them training in discriminating internal drive state) displayed quite different behavior when tested and trained under identical conditions to those described in a previous experiment (2) which followed the basic Spence and Lippitt design. The *S*s in question had been subjected to a complex food and water deprivation schedule between the ages of 30 and 90 days (Table 1). When these *S*s were trained under hunger and tested under thirst, it was found that only 3 of 10 continued to go to the right-hand goal box which was appropriate during the training series. Animals, previously tested, which had had no previous history of deprivation,[2] had gone in 8 of 9 cases to the previously rewarding goal box on the first test trial. This difference between the

[1] These experiments were conducted in the psychological laboratories at the University of California. Professor David Krech served as faculty advisor. The author is also indebted to Professor E. C. Tolman for many helpful criticisms and comments.

[2] These *S*s were of the same stock, were trained and tested in the same maze, and were exposed to the same experimental procedures. These "normal" *S*s, as they will be referred to hereinafter, were the control group in the cited experiment (2).

TABLE 1

DEPRIVATION SCHEDULE

Day*	Amount of Deprivation Hours	Drive
1	6	Hunger
2	6	Thirst
4	6	H + T
6	12	Thirst
7	12	Hunger
9	6	Hunger
	12	H + T
12	24	Hunger
14	24	Thirst
17	6	Thirst
	18	H + T
19	36	Thirst
22	36	Hunger
26	48	H + T
30	18	Thirst
	6	H + T
32	18	Hunger
	6	H + T
35	60	Hunger
38	60	Thirst
42	24	Hunger
	24	H + T
46	24	Thirst
	24	H + T
50	24	Hunger
	48	H + T
55	24	Thirst
	48	H + T

* Day given refers to the commencement of any particular deprivation period.

two groups was significant at the 2% level of confidence.[3]

The *Ss* experiencing early deprivation (hereinafter referred to as Group X) also behaved aberrantly on the two subsequent test trials under hunger, making only 50% and 60% responses to the right-hand goal box where they had previously received 14 food reinforcements. This contrasts with the 100% right-hand responses made by the group subjected to "normal" rearing practices, this difference being significant at the 1% level of confidence. The fourth test trial was again made under thirst motivation. Only 1 (10%) of the *Ss* of Group X made a choice to the right-hand goal box.

[3] The tests of significance used herein are based upon *t* scores based upon standard errors of proportions.

If exposure to the deprivation regimen had in some fashion handicapped the *Ss* of Group X to the extent that they could not learn *any* problem as adults, the obtained results could be attributed to fluctuation about the chance expectancy of 50% in a maze with two options. The total percentage of choices for the right-hand goal box was 37.5 on all four test trials, this falling short of the 10% level of confidence in differing from chance.

In contrast to what might be termed a *deprivation* hypothesis, it might be argued that a different explanation could account for the behavior of Group X. Thirst is commonly recognized as being a stronger drive than hunger. The *Ss* of Group X were subjected to *objectively equivalent* periods of food and water deprivation in terms of hours. If thirst were *subjectively* more severe for the *Ss*, the water bottle which alleviated their thirst after water deprivation might be more strongly cathected (or have greater secondary reward value) than the food can. When, during the training series under hunger, the *Ss* were exposed to the water bottle, it might have a secondary reward value which counteracted in part, at least, the primary reward value of the food in the opposite goal box and the weaker secondary reinforcement value of the food can. Thus a cathexis for the water bottle resulting from their previous history of deprivation might account for the behavior of Group X. Some support for this *cathexis* hypothesis was derived from the fact that the *Ss* of Group X made 80% responses to the goal box containing the water bottle on the two test trials under thirst motivation, this being different from chance at better than the 5% level of confidence.

It was decided to test two groups of

Ss in an effort to determine which, if either, of the alternative hypotheses sufficed to explain the behavior of Group X. The first, Group A, was subjected to exactly the same pre-deprivation schedule as was Group X (Table 1) and the same experimental treatment. A second, Group B, was given identical pre-experimental and experimental treatment as Groups X and A with one exception, i.e., no water bottle was placed in the left goal box. Thus, no possible cathexis for the water bottle could be developed. If these Ss made chance responses on subsequent test trials, their behavior would be interpreted as confirming the *deprivation* hypothesis. If, on the other hand, they were able to learn the location of food during the training series and responded to it significantly better than chance on the test trials, and the Ss of Group A displayed behavior similar to that of Group X, the *cathexis* hypothesis would be more tenable.

The unusual behavior of Group X raised a more general question regarding the effect of early experience upon adult maze learning behavior in rats. The majority of rats are reared in cages under conditions which permit relatively little deprivation (1). If pre-experimental deprivation is sufficient to affect significantly subsequent behavior, it appears plausible that other sorts of noncustomary pre-experimental procedures might likewise affect experimental behavior. In consequence, it was decided to investigate the effects of early exploratory behavior to determine if this, as well as early deprivation, affected the ability of adult rats in the present type of experiment.

Two further groups of Ss were run. One, Group C, was subjected to the same pre-experimental deprivation regimen as Groups A and B. After each period of deprivation, these Ss were required to locomote in the environment to find food and/or water to alleviate their stage of physical need. Another, Group D, was similarly required to explore the environment before finding food or water, and was trained and tested under conditions identical to those for Groups X, A, and C. During rearing they had ready access to food and water at all times. Group D was comparable to the "normal" group previously alluded to, in terms of lack of pre-experimental deprivation, but had the additional experience of environmental exploration.

TABLE 2

EXPERIMENTAL DESIGN

Group	Early Deprivation	Early Exploration	Water Bottle in Left Goal Box
"Normal"	No	No	Yes
X	Yes	No	Yes
A	Yes	No	Yes
B	Yes	No	No
C	Yes	Yes	Yes
D	No	Yes	Yes

The varying conditions to which various groups of Ss were subjected may be found in Table 2.

PROCEDURE

The procedure during the maze familiarization, training, and test periods, and the maze used in the experiment proper were identical with those previously reported (2). Only additional information and variations from the previous experiment will be reported in the present account.

Subjects.—Sixty-five male and female rats from the University of California colony were obtained immediately after being weaned and were between 23 and 28 days of age. They were pigmented rats of the Tryon M × M strains. Their ages at the beginning of the pre-experimental training ranged from 28 to 33 days. Prior to the beginning of pre-experimental training they were kept in cages having a constant supply of water and the standard University of

California dry mash. There were usually four *Ss* to a cage but in some instances only three were confined in the same cage. One *S* died during this five-day period. The *Ss* were divided by splitting litters into four groups (A, B, C, and D) of 16 each immediately prior to preexperimental training.

Pre-experimental exploratory apparatus.—Groups C and D were allowed to explore in a noncage environment. This consisted simply of an elevated path leading to a table. Two identical situations were presented to the two groups. The two tables were 30 in. by 48 in. in surface area and were covered with wood shavings. On each was a wire cage containing four water bottles. These were mounted inside the cage with the nozzles protruding on the outside. A porcelain bird bath containing dry mash was placed next to each water nozzle.

The paths leading to each table were part of a dismantled elevated maze and were 48 in. long. They were 2 in. wide and were painted black. They stood, as did the tables, 32 in. above the floor. An additional detour section was sometimes added to each path to make the *Ss* change direction on the way to the table. This detour was of the same width as the rest of the runway and consisted of an initial 12-in. straightaway, a 12-in. section at right angles, a 12-in. length continuing in the original direction, and another 12-in. section at right angles paralleling the second section, and a final 12-in. section continuing in the original direction and at right angles to the preceding one. In no instance were the *Ss* required to make a choice as to which path to take—the sole requirement being to proceed down the pathway to the table. The first turn on the detour section was to the right and left an equal number of times.

The location of the tables in the room was varied from trial to trial and the direction of the paths to the tables, the angle of approach, and the absolute distance from the starting point to the location of reward on the table varied from trial to trial so that in no case was the same configuration of pathways leading to food and water presented to the *Ss* in the 20 exploratory trials. All light and noise cues were constant during this period of training.

Experimental apparatus.—Two identical tunnel T-mazes were constructed. Groups A and B were tested in one, and Groups C and D in the other. These mazes were identical to those reported previously (2).

Food was found in the right-hand goal box in either a porcelain bird bath or can identical to that previously found in the home cages and exploratory table. The water bottle was mounted in the left-hand goal box for all groups except B. In all cases its height and appearance were similar to the ones previously experienced by the *Ss*.

Pre-experimental training in deprivation.[4]—Three of the four groups (A, B, and C) were subjected to a deprivation schedule identical to that imposed upon the *Ss* of Group X (Table 1). All three groups were deprived of water alone in their home cages by removal of water bottles; of food alone by being transferred to deprivation cages mounted directly below the home cages. Deprivation of food and water simultaneously also occurred in the deprivation cages. For *Ss* of Groups A and B water deprivation was ended by the insertion of a water bottle into the home cages, deprivation of food alone and food and water combined was terminated by the transfer of the *Ss* back to the fully stocked home cages.

Group D was never deprived, but when *Ss* of the other three groups were shifted to deprivation cages, *Ss* of Group D were transferred to cages which always contained food and water and were located directly below their home cages. This procedure was followed in order to equate the amount of handling for the four groups. All *Ss* were extremely docile.

Exploratory training.—The *Ss* of Groups C and D received 20 exploratory experiences. These occurred immediately after Group C had been subjected to deprivation as indicated in Table 1. On the first exploratory trial the deprived *Ss* of Group C and the nondeprived *Ss* of Group D were placed individually on the end of the straightaway nearest the table and coaxed onto the table. All *Ss* would go readily down the elevated paths to the tables after four trials. After *Ss* of Groups C and D reached the top of the table on which food and water were placed, they were allowed to remain there for 24 hr. The *Ss* of Groups C soon ate and/or drank upon first arriving on the table top, depending on their drive state. The *Ss* of Group D customarily did not eat or drink immediately upon arriving at the table top, but since they were there for 24 hr. they did eventually eat and drink there.

Initially, *Ss* of Group C were trained on one table and *Ss* of Group D on another. With the onset of sexual maturity, it was deemed advisable to separate the males and females of these two groups during the exploration period. Subsequently the males of Groups C and D were trained on one exploratory apparatus and the females of the two groups received parallel exploratory experience on a separate apparatus. *Ss* of both groups thus had similar exploratory experiences.

[4] Mr. Louis Stone assisted the author in the pretraining of the *Ss* and handled Groups C and D in the experiment proper.

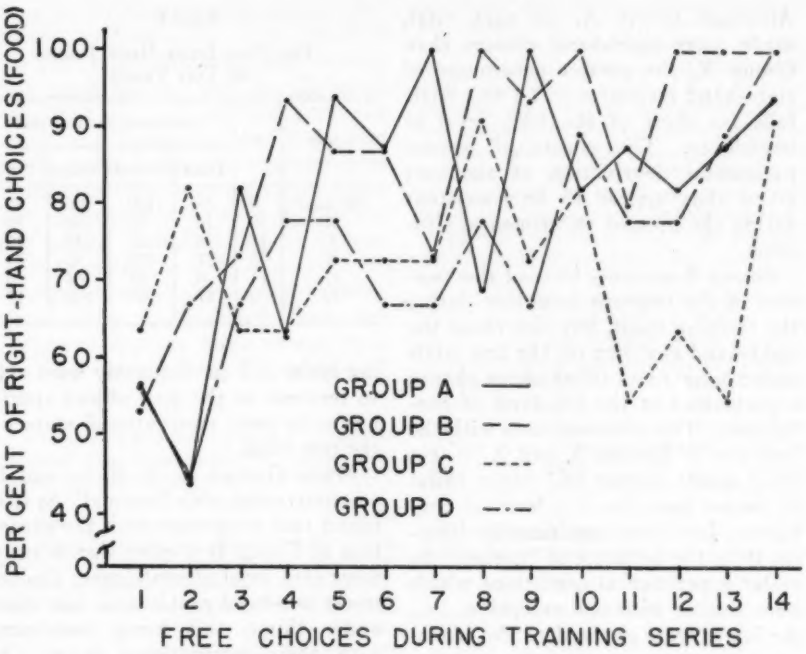

FIG. 1. Percentages of right-hand turns to the goal box containing food in the training series

Preliminary maze training.—This again was similar to that previously described (2). After the elimination of those *S*s which displayed position biases by three consecutive choices to the same alley, 16 *S*s were left in Group A, 15 in Group B, 11 in Group C, and 12 in Group D.

Experimental training and test trials.—These replicate exactly the procedure previously reported with the exception of the *S*s of Group B which did not find the water bottle in the left-hand goal box. When *S*s of Group B chose the left-hand goal box during the training trials, they were detained there for 2 min., as were the groups exposed to water. No *S* of Group A, C, or D was observed drinking from the water bottle during the training series, and those *S*s choosing the left-hand goal box during the test trials were removed before having an opportunity to drink.[5]

[5] The nozzle of the water bottle in a cage containing three *S*s of Group D was accidentally blocked after the first test trial. These *S*s had gone to the left goal box on the first test trial under thirst. Upon subsequent deprivation of food they went to the right-hand goal box. Their behavior conforms to that of the other *S*s of Group D. They are not, however, included in the results because of the disruption in the testing sequence.

RESULTS

An inspection of the free choices made during the 14 rewarded trials of the learning series (Fig. 1) indicates that Groups A, B, and D "learned," i.e., they made an increasing number of choices to the right-hand goal box containing food. The behavior of Group C was much more erratic, chance responses occurring on Trials 11, 12, and 13.

The behavior on each of the four test trials of Groups X and A, which were exposed to similar pre-experimental and experimental conditions, is similar, no significant differences on any test trial being found (Table 3).

Although Group A, on each trial, made more right-hand choices than Group X, the greater percentage of right-hand responses on all test trials falls far short of the 10% level of confidence. The effects of pre-experimental deprivation of the sort given thus appear to be consistent within the present experimental situation.

Group B not only learned the location of the relevant incentive during the training trials, but also chose the right-hand goal box on the test trials under hunger and thirst above chance expectations at the 1% level of confidence. This also contrasts with the behavior of Groups X and A on test trials under hunger and under thirst at better than the 1% level of confidence, but is not significantly different from the behavior of "normal" *Ss* under experimental conditions which were similar with one exception, i.e., the latter were exposed to the irrelevant incentive during the training trials.

The behavior of Group C was quite different. Every *S* chose the left-hand goal box on test trials under thirst, thus displaying perfect latent learning, inasmuch as reward had never been previously received at that locus. However, on test trials under hunger, their behavior did not differ significantly from chance. These *Ss* were able to latent learn but displayed little, if any, evidence of ability to learn under reinforcement.

The *Ss* of Group D behaved quite differently from the other *Ss* on the test trials. On test trials both under hunger and under thirst their choices of the relevant goal object differed significantly from chance at better than the 1% level of confidence. In other words, they were able to learn the location of both the relevant and irrelevant incentives during the training series and subsequently were able to respond to the goal object appropriate to their motivational state on the test trials.

TABLE 3

PER CENT RIGHT-HAND TURNS ON TEST TRIALS

Group	*N*	Motivation on Test Trials			
		Thirst	Hunger	Hunger	Thirst
"Normal"	9	89	100	100	89
X	10	30	50	60	10
A	16	38	56	75	19
B	15	87	100	93	73
C	11	0	55	64	0
D	9	11	89	100	11

When Groups X, A, B, C, and D are contrasted with "normal" *Ss* it is found that all groups with the exception of Group B display significantly more (1% level of confidence) choices to the left-hand goal box on test trials under thirst, this being consonant with their motivational state. On test trials under hunger, Groups X, A, and C made significantly fewer (1% level of confidence) appropriate choices to the right-hand goal box containing food than did Groups B, D, and "normals."

On test trials under hunger, *Ss* of Groups X, A, C, and D chose the right-hand goal box significantly more frequently (2% level of confidence or better) than they did on test trials under thirst. "Normals" and Group B were found to continue to the right-hand goal box in which they had previously been rewarded regardless of their motivational state on test trials.

DISCUSSION

The initial query as to the underlying reason for the unusual test behavior of Group X is answered by the differential behavior of Groups A and B. The *deprivation* hypothesis is

negated by the learning ability displayed by Group B. The choice of the previously irrelevant goal box on test trials under thirst by Group A and their failure to respond appropriately to the right-hand side on test trials under hunger is interpreted as confirming the *cathexis* hypothesis. The cathexis for the water bottle was not strong enough, however, to prevent *Ss* of Group A from learning the location of food during the training series (Fig. 1) although their performance appears more erratic than that of Group B or "normals" (2, Fig. 1).

The test performance of Groups C and D indicates that pre-experimental experience in extramaze exploration has marked effects upon subsequent behavior. Exploration under deprivation in the present experiment enhanced the effects of deprivation, the behavior of Group C being consistently more oriented toward the irrelevant incentive than that of Group A, which had similar deprivation but no extracage experience.

Group D, on the other hand, had early exploratory experiences under conditions of satiation for food and water. Such experience did not adversely affect *Ss'* ability to learn on the training series under hunger, although this learning was slower than that of Group B. However, on test trials under hunger and thirst, the *Ss* of Group D were able to respond appropriately in accordance with their motivational state.

The differential choices made by the *Ss* of Groups X, A, C, and D on test trials under hunger as contrasted with those made under thirst is interpreted as indicating that these *Ss* (*a*) were able to differentiate between internal drive states of hunger and thirst and (*b*) had discriminatingly perceived the irrelevant incentive during the training series.

In a previous paper (2) the question was raised as to whether rats which had never experienced acute states of hunger or thirst could differentiate between these internal drive states. Group D in the present experiment was never subjected to food deprivation prior to the learning series and did not experience acute thirst prior to the first test trial. These *Ss*, nevertheless, were able to respond appropriately to the incentives upon being motivated for them. Thus, the inference can be made that they were able to discriminate between internal states of hunger and thirst without previous training in deprivation.

Not only do the quantitative data regarding the appropriate choices on test trials under thirst indicate that *Ss* of Groups X, A, C, and D perceived the irrelevant incentive during the training trials, but their qualitative behavior on test trials also indicated their awareness of the location of the water bottle. On their approach to the left goal box, they could not, of course, see into it. The nozzle of the water bottle was on the right-hand side of the goal box and could not be seen until actual entry into the goal box had been made. These *Ss*, however, went directly to the right-hand corner toward the nozzle in most purposeful fashion and had to be removed immediately before they could drink.

These results raise a number of questions concerning the conditions requisite for the learning of irrelevant incentives. Two conditions demonstrably enhance this ability: (*a*) pre-experimental deprivation of the sort described above, and (*b*) pre-experimental exploratory experience. The necessary amounts of such experiences and the stage in the developmental history of *Ss* when such experiences would be most effective cannot be answered on the basis of the present data.[6]

Summary

Rats (Group X) with an early history of food and water depivation were found to behave quite differently from nondeprived *Ss*, when trained and tested in an experiment similar to that of Spence and Lippitt's (6). Rats subjected to early deprivation were able to respond appropriately to a previously irrelevant water bottle when made thirsty, but behaved at a chance level when hungry, despite 14 reinforcements of food during the training series under hunger motivation.

Two possible explanations for this behavior were considered: (*a*) the early regimen of deprivation might have impaired subsequent learning

[6] Mr. Robert Vineberg has recently completed a series of experiments under the cosponsorship of Professor Howard H. Kendler and the author in which a variety of early exploratory experiences were followed by irrelevant incentive learning under similar experimental conditions.

ability, or (*b*) early deprivation might have led to the establishment of cathexes for the water bottle. Two groups of *S*s were run to test these alternative hypotheses. Both groups were subjected to early, pre-experimental deprivation, identical training, and testing with one exception—namely, the water bottle (irrelevant incentive) was not in the irrelevant goal box during the training series for one group. The group not finding the water bottle responded to the locus of previous reward on all test trials regardless of motivation, thus disproving the deprivation hypothesis. The group exposed to the irrelevant incentive behaved similarly to *S*s of Group X on test trials. That is, they preferred the side containing the water bottle when thirsty and did not respond appropriately to the locus of previous reward when hungry. This differential behavior on the part of the two groups support the cathexis hypothesis.

Two additional groups were tested. Both were given training in exploring the environment. One group explored after being deprived, the other explored while satiated. Both groups, when trained and tested under conditions similar to those imposed on the previous groups, were able to learn the location of the irrelevant incentive. The group subjected to early deprivation did not respond to the side of previous reward when motivated for it during test trials. The satiated explorers made responses appropriate to their motivational state on test trials under hunger as well as under thirst.

Both food and water deprivation of the sort given and early exploratory experience were found to enhance the *S*s' ability to learn the location of an irrelevant incentive.

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PLACE AND RESPONSE LEARNING IN THE WHITE RAT UNDER SIMPLIFIED AND MUTUALLY ISOLATED CONDITIONS

CHARLES W. HILL[1] AND LELAND E. THUNE
Vanderbilt University

The purpose of this study is to simplify and clarify a controversial problem that has recently emerged: that of the relative effectiveness of place and response learning. The first anticipation of this problem seems to be contained in the 1944 study of Blodgett and McCutchan (1) concerning the choice-point behavior of the white rat in a double-T maze. In 1946 Tolman and his colleagues, Ritchie, Kalish, and Gleitman (9, 10, 11, 12, 13, 14, 15), initiated a series of experiments the results of which indicated, in general, that (*a*) the white rat employs both place and response methods when the situation so permits, and (*b*) place learning is more efficient than response learning in terms of economy of learning time. However, Blodgett and McCutchan (2, 3, 4, 5), in several later studies, apparently refute the findings of Tolman *et al.* and conclude that response learning is superior to place learning. Gentry, Brown, Lee, and Kaplan (6, 7) in two consecutive experiments, also produced results that were opposed to those of Tolman. Finally, Kendler and Gasser (8) found some faint support for place learning; while Wilcoxon and Waters (16) were unable to discover any indications of it.

A detailed examination of the above studies reveals (*a*) that there is positive, conflicting evidence on both sides of the controversy, and (*b*) that there has been a wide variety of experimental designs and procedures employed. It is believed that much of the conflict and confusion is attributable to the lack of standardization among the various methodologies. In the early stages of investigation, it would seem preferable to strip the problem down to its simplest form, and to search for a definite answer there before attempting to generalize.

Another possible source of confusion is the failure in most of the studies to isolate the two types of learning conditions under investigation. As long as both are available to the rat, it would seem that there would be the opportunity for one either to support the other, or for one to conflict with and inhibit the other. A second requirement, then, for a basic experiment would be the mutual isolation of the place and response learning conditions to the greatest possible extent.

All the studies which have been discussed above may be divided into two general categories according to their conception of spatial orientation. First, there are those which consider spatial orientation as the ability to use spatial cues in making the correct turn at the choice point; and, second, there are those that consider it to be the ability to employ spatial cues in selecting the correct shortcut. These two abilities may well be separate and discrete, or have very little overlap; and therefore it would seem best to keep them apart, at least in the early investigations. Henceforth, this study will be concerned solely with the first category, which appears to be

[1] Now in the office of the AC/S, G-1, Department of the Army.

the simplest. Only 10 of the 16 studies mentioned above belong in this category: five by Tolman *et al.* (9, 12, 13, 14, 15), and five by Blodgett and McCutchan (1, 2, 3, 4, 5).

Blodgett and McCutchan (1, 2, 3) were the first to investigate the present problem, although at the time they were primarily interested in "anticipation." The presence or absence of spatial cues is specified by them as being of critical significance in the explanation of anticipation. However, when Tolman and his colleagues report some positive results in this area, Blodgett and McCutchan undertake to refute them.

The second spatial learning study of Tolman *et al.* (12) and the refutation by Blodgett and McCutchan (4) were almost identical in design and purpose. Both used a one-piece maze that permitted the influence of intramaze cues. Both employed the correction method which tends to inhibit or confuse both place and response learning. In Tolman's study, the fixed maze in a bright light permitted a conflict of place and response learning in the response group that favored the place group; while, conversely, the one-piece maze of Blodgett and McCutchan's study, even though it was rotated, permitted a continuous repetition of the same problem for the response group which militated against the place group.

The next two studies in the Tolman series (9, 13) involved simply an investigation into the efficiency of place learning without utilizing response learning as a control. Study III (9) compared one-place against two-place learning; while Study IV (13) compared the opportunity for place learning against complete naiveté.

A shift to the noncorrection method was made in Study V of the Tolman series (14), which method was continued in all their subsequent experiments. Both place and response groups were trained under identical conditions prior to a rotation of the maze, thus permitting the possibilities of spatial conflict, response conflict, double-place learning, and double-response learning.

The 1948 study of Blodgett and McCutchan (5) provided an extramaze cue that was actually more of a direct stimulus than a spatial cue. All other possible extramaze cues were disrupted through continual rotation of the maze. During the initial phases of the experiment there existed the opportunity for both place and response learning, which later were thrown into conflict.

The 1949 study of Tolman and Gleitman (15) also permitted this conflict of place and response learning. The results of blinding indicated that response tendencies are acquired rather than a complete dependence upon double-place learning, but there still remained the possibility that concomitant place learning can partially inhibit response learning, and vice versa.

In order to avoid the many sources of conflict and confusion that existed in the above-mentioned studies, the learning situation in the present study was restricted to the single choice point, and maximum effort was made to insure the mutual isolation of place and response learning conditions.

The following six controls were established in order to attain the objectives set forth above: (*a*) the employment of the noncorrection method for the running of the rats; (*b*) the minimization of all possible intramaze cues; (*c*) the reduction to a minimum of the interaction between place and response learning conditions; (*d*) the equalization of the variability within the problem for both place and response learning groups; (*e*) the provision of positive, extramaze cues for the place learning group rather than direct stimuli; and (*f*) the elimination of opportunities for double-place and double-response learning. With these controls it was hoped to strip the place versus response learning problem down to its barest essentials; yet, at the same time, to insure that the final comparison was actually one between pure place learning and pure response learning.

No specific hypothesis was advanced, the object of the experiment being to render an impartial decision on the relative effectiveness of the two types of learning under the above-described conditions.

Procedure

The problem that was presented to the rats involved a single choice of direction to obtain a food reward upon an elevated, single choice-point

maze with certain variations of design as described below. For the place learning group, the reward was always found in the same room location regardless of the direction of approach to the choice point or the position of the wrong alternative. For the response learning group, the reward was always found by turning in the same direction at the choice point regardless of the room location or the position of the wrong alternative.

Subjects and apparatus.—Fifteen naive, male albino rats, approximately 6-mo. old, were employed as *S*s. The test trials were conducted in a black-painted, interior room, 6 by 8 ft., with a shaded, 15-w. light suspended from the center of the ceiling to within 6 ft. of the floor. A one-way-vision screen, made of cheesecloth, hung to the floor from a large hoop which encircled the light. In diagonally opposite corners of the room, a 7½-w. light and a dull electric buzzer stood at the height of the maze runways.

A black, elevated maze, 15 in. high, was built in three 20-in. sections, which were connected at the choice point by an octagonal platform thus permitting angles of 45°, 90°, and 135° between adjacent sections. The starting and end platforms were removable, and the end platforms contained countersunk food-cups. This maze was set up in the experimental room with the choice point directly beneath the center light.

Preliminary training.—Seven days prior to the first test trial all rats were placed upon a 24-hr. feeding schedule of 15 gm. of Purina lab chow. During the feeding period, which was gradually decreased during the week from 2 hr. to 1 hr., the rats were removed from their home cage and placed in individual cages. Water was available at all times in the home cage, but none was present during the feeding period.

Five days before the first test trial the maze was assembled in the form of a simple T outside the experimental room. Each *S* was given four unre-enforced trials per day for three successive days to provide familiarity with the T-maze situation. On the next two days a straight runway was employed and a food reward introduced; and each *S* was given three trials per day in order to acquaint him with the maze-food situation without choice-point learning.

Upon completion of the preliminary training the *S*s were divided into two comparable groups of seven and eight each, in which turning tendency, timidity, and willingness to eat were roughly equated. The eight *S*s were arbitrarily designated as the place learning group, and the seven *S*s became the response learning group. The place group shortly was reduced to seven, as one rat failed to meet the criterion time on the starting platform decribed below.

Test trials, general.—The test trials began on the eighth day and continued for 14 successive days. They consisted of four consecutive trials per day for each *S*, with the maze design altered on each trial in accordance with patterns which are described below. Also, between trials the three maze sections were reversed end for end and rotated among themselves in a systematic manner designed to disrupt the influences of various intramaze cues. This rotation insured that, for any given trial, both alternative runways available to *S* at the choice point had been traversed on the preceding trial.

It was decided that any *S* which failed to move from the starting platform within 5 min. would be removed from the experiment. A pellet of lab chow was placed in the concealed food-cup on the appropriate end platform to serve as the reward. The *S*s were permitted to eat for 10 sec. each time they made the correct choice and arrived at the goal. They were then removed from the maze and placed in a special cage outside the experimental room while the maze was being rearranged for the next trial. The interval between trials was approximately 1 min. Upon completion of the fourth daily trial *S* was returned to the home cage.

The noncorrection method was employed, the rats being returned to the starting platform if

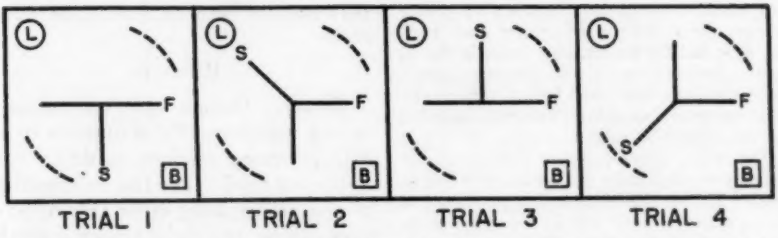

FIG. 1. The maze designs for the four daily trials of the place group. Only the RLLR series is shown. The LRRL series may be traced by starting at Trial 3 above, then Trial 4, Trial 1, and lastly Trial 2.

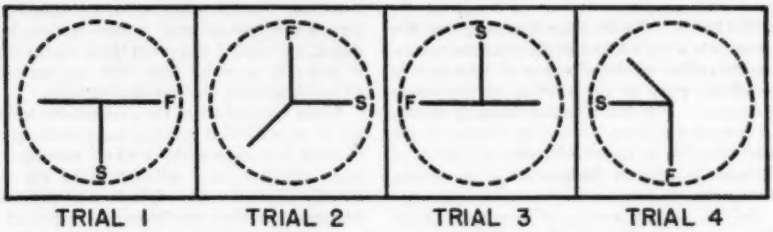

FIG. 2. The maze designs for the four daily trials of the response group. Each of the maze positions was rotated 90° about the four sides of the room between successive rats on any one day, and for each individual rat from one day to the next.

they ran at least halfway down the wrong runway. On each successive day the place and response groups were alternated in being the first to run the four trials.

Test trials, place group.—The maze designs for the four daily trials are shown in Fig. 1. The position of the reward platform remained fixed in relation to the room throughout the performance of the place group. Two counterbalanced series of correct turns, RLLR and LRRL, were provided for the four daily trials. Figure 1 depicts the RLLR series only, but describes how the other series can be determined. The two series were alternately presented to successive *S*s on any one day, and also alternated for each *S* from one day to the next.

The simple T was modified to a Y design on alternate trials in order to minimize the possibilities for response learning, by continually presenting two turning choices neither of which had been identically available on the preceding trial. As this alternation was always presented in the same order, the four trials combined must be considered as the smallest balanced unit of practice.

The panels of the one-way-vision screen were rolled up on the hoop out of the way, except for two which were slanted into diagonally opposite corners of the room as shown in Fig. 1. The *E* retired behind one of these screens after placing *S* upon the starting platform for each trial. The light and the buzzer, also shown in Fig. 1, were connected to a constant-rate circuit breaker, which caused them both to turn on and off about once a second. These were intended to serve as positive, extramaze cues.

Test trials, response group.—The maze designs for the four daily trials were as shown in Fig. 2. The position of the reward platform was always such that a 90° turn to the right was required at the choice point. Between each trial, the entire maze was rotated through a 90° angle so that the starting platform continually held the position occupied by the food platform on the preceding trial. This procedure contributed to the minimizing of place learning, by preventing the correct "place" of one trial from becoming either the right or the wrong alternative on the succeeding trial.

Further efforts to reduce place learning consisted in the rotation of the Trial 1 starting position about the four sides of the room for each successive *S* on any one day, and for each individual *S* from one day to the next. Figure 2 shows only one of the four possible positions for each of the trial designs.

During the four daily trials, the angle of the wrong-choice runway was altered from a right angle, to an obtuse angle, to a right angle, to an acute angle. This was done to provide the response group with a continually changing intramaze design comparable to that presented to the place group. As this variation was always presented in the same order, the four daily trials combined must be treated as the smallest, balanced unit of practice.

All panels of the one-way-vision screen were dropped to the floor, thus completely enclosing the maze as shown in Fig. 2, and minimizing the irregularities of the room as sources of extramaze cues. The *E* remained outside this screen while each *S* was performing. The flashing light and buzzer were not in operation for the response group.

RESULTS

Errors.—During the test trials a record was kept of the number of errors, or wrong choices, made by each *S* on each trial. For the computation of the group daily error scores, however, these multiple errors were ignored and individual trials were scored either "1" for one or more

errors, or "0" for no errors. A daily individual error score was then obtained from the sum of the four trial scores; and finally the group daily error score was computed as the mean of the individual daily scores for all Ss of each particular group.

The group learning curves in terms of daily error scores are shown in Fig. 3. It will be seen that, after the first three days of exploratory confusion, the errors of the response group dropped below chance expectancy, and well below those of the place group, and remained thus for the final 11 days. The place group remained above chance for six days, after the first three, then dropped below and remained below for the final five days.

Time.—Records were also kept of the time consumed from placement of the rat upon the starting platform until its arrival at the food platform. Individual daily mean time scores were computed from the individual trial times. Daily mean time scores were then obtained for each group.

The group learning curves in terms of daily time scores are shown in Fig. 4. It will be noted that these curves follow the same general trends as the curves of errors. However, there are two major differences that are deemed worthy of interest: (*a*) the difference between the group curves during the period Day 4 to Day 9 is relatively much smaller in the time curves than in those of errors; and (*b*) there is a merging of the group time curves during the period Day 10 to Day 14, whereas the error curves continue to remain apart.

Tests of significance.—The differences between the place group curve and the response group curve in terms of errors were tested for significance at all daily points by *t*. Differences significant at the .001 level were obtained from Day 4 through Day 7, and at the .01 level for Days 8, 9, 11, and 13. These establish the superiority of the performance of the response group over that of the place group during this period of learning with a high degree of probability. Even though significant differences were not obtained on Days 10, 12, and 14 ($p > .10$, .10, .06, respectively) it is believed that, with the support of the consistent trends of both group error curves, the more efficient performance of the response group is still evidenced.

Since the noncorrection method was employed, the meaningfulness of the individual trial times was diminished, because the clocked

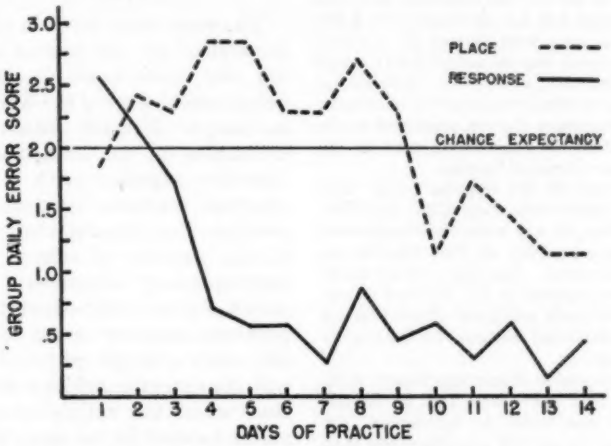

FIG. 3. Learning curves in terms of errors for both place and response groups

FIG. 4. Learning curves in terms of time for both place and response groups

time included the additional time required to return *S* to the starting platform after each incorrect turn. Therefore, no significance tests were applied to the differences between the group time curves, and only their general trends are utilized as auxiliary data to the more rigorous error scores.

The differences, within each group, between the error scores for the first and the last day of the experiment were tested for significance by *t* for related measures. The difference of 2.14 for the response group was significant at the .001 level ($t = 8.23$). The difference of .72 for the place group was not significant ($t = 1.20$). The fact that some learning took place within the response group may be accepted with a high degree of probability. For the place group, however, the consistent trend of the error curve from Day 10 through Day 14, supported by the similar trend in the time curve, must be appealed to for evidence of learning.

The distance of the response group error curve below chance expectancy (2.00) from Day 4 through Day 14 was tested for significance, and a chance probability of .0164 (Day 8) was the largest obtained. Therefore, all the differences may be accepted at the 2% level of confidence. This lends additional support to the previously established evidence for learning by the response group.

The distance below chance expectancy of the place group error curve from Day 10 through Day 14 was then tested for significance. On Days 10, 12, 13, and 14 the differences were significant at the .02, .10, .02, and .05 levels, respectively. The difference on Day 11 was not significant. The distance above chance expectancy obtained by the place group from Day 4 through Day 9 was tested for significance, and only the difference on Day 4 was found to be significant ($p = .001$, $t = 6.14$). However, the fact that the differences were above chance on all six days suggests that some factor other than chance, possibly the learning of an inadequate solution, was operating.

DISCUSSION

The results set forth in the preceding section are interpreted as indicating that, with organisms at the developmental level of the white rat and in simple learning situations, the utilization of an immediate simple stimulus together with a constant physical response is more efficient, primitive, or natural than that of a spatial pattern of stimuli requiring discriminatory responses. In other words, a more rapid adaptation to the problem situation seems to be possible when a single physical response will consistently achieve satisfaction than when the satisfying object remains located in the same place, but requires different physical responses

to reach it. In the first instance there is a fixed form of behavior to be associated with a slightly varying stimulus-complex; while in the second case, the intrarelationships of the stimulus-complex remain fixed and the behavior of the organism must vary in accordance with the changing relations between the organism and the stimulus-complex.

Although both phenomena can be explained in terms of either associative or cognitive concepts, it is believed that the relative differences in efficiency of learning that have been obtained in this experiment can be more plausibly explained by an association theory of learning.

If the cognitive theory were the more appropriate, there should have been no difference between the performance of the place group and that of the response group. In both cases there was a constant, total situation available to serve as a general stimulus-complex, upon which could be developed the necessary readinesses and expectations. The same demand, the hunger drive, was present in both instances, which should have resulted in equal motivation, equal highlighting of the keypoints, and given equal value to the food as a reward. If an expectancy can be formed, with a certain degree of facility, to the effect that "in this situation a right turn will produce the reward," then the other hypothesis to the effect that "in this situation by proceeding to such-and-such a place will produce the reward" should be capable of formulation with equal facility. In other words, the rats of the response group, upon experiencing their particular maze-drive situation, would acquire an expectancy concerning the location of the reward in terms of the direction of the turn at the choice point; and the rats of the place group would develop an hypothesis concerning the location of the reward in relation to the pattern of spatial cues.

Thus, it would seem that the place group's total situation of maze, drive, and spatial cue pattern would produce an expectancy with as much facility as would the response group's total situation of maze, drive, and direction of turn. There should be little or no difference between these two cognitive processes, either in difficulty or in the time required for their formulation. Of course, the objection could be made that the sign-gestalt of the place group is the more complex, and therefore more difficult and time consuming. The cognitive theories, however, do not emphasize such a time differential, nor do they provide any basis for quantitative evaluation. If the organism possesses the capacity for spatial means-end-relations, as well as the capacity for physical response relations, then it is capable of developing either of the sign-gestalts in question, with no summation or building activity implied in either case.

If, on the other hand, an association theory of learning should be applied to this problem, it would seem that the connection of a general maze-drive situation to a constant, physical response should be an easier or more efficient process than the multiple association of several specific spatial patterns with their corresponding directional movements. In the former case, there is but a single association to be established and re-enforced. In addition, all the activities involved in the introduction of the rat to the maze situation could develop into a goal gradient that would always culminate in the same correct response at the choice point. In the case of multiple associations, each specific

spatial pattern must be perceived and discriminated, and then the appropriate directional response must be evoked and connected.

Therefore, in associative terms, the learning of the correct solution by the response group would be due to the establishment and strengthening of the single association between the simple stimulus of being placed upon the maze while hungry and the simple response of running forward a short distance and then turning to the right. This same associationism would also hold that, in the case of the place group, the successive establishment of several independent connections would be necessary. Each maze situation involving a unique relationship between the spatial pattern of cues and the starting point would have to be associated with the corresponding angle and direction of turn.

The association theory thus provides a basis for making quantitative predictions regarding the relative difficulty of these two learning situations. Whereas the response group found it necessary to develop only one association in order to successfully solve their problem, the place group was required to establish four different associations, and each connectional process was, to a certain extent, independent of the other three.

It is therefore concluded that, in terms of associative principles, the problem of the place group was more complicated and difficult than that of the response group, and should have required more time and practice for its adequate solution. Inasmuch as there was a significant difference obtained between the performances of the two groups in favor of the response group, this experiment contributes additional support to the thesis that response learning is superior to place learning, as well as to the greater comprehensiveness of the associative theory of learning.

Summary and Conclusions

A controversy has recently developed in the literature as to whether place learning or response learning is the most primitive or natural in the white rat. The present study was designed to eliminate or minimize the many sources of conflict and confusion that exist among the previous experiments.

A problem involving a single choice point upon an elevated maze was presented to two groups of seven rats each. One group, operating in a homogeneous environment, was required continually to make a right turn in order to solve the problem; while the other group, with the help of positive spatial cues, had to constantly run to the same room location. All the rats were motivated with hunger to the same degree and rewarded with food.

Test trials were continued over 14 successive days. On Day 4, the performance of the response group dropped below chance expectancy, in terms of errors, and remained below for the remaining 11 days. The performance of the place group, on the other hand, remained above chance until Day 10, at which time it also dropped below, but still stayed higher than the response group.

The obtained results appear to give support to an associative theory of learning rather than to a cognitive theory.

(Manuscript received July 30, 1951)

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GRADIENTS OF DRIVE STIMULUS (S_D) INTENSITY GENERALIZATION [1]

HARRY G. YAMAGUCHI [2]

Institute of Human Relations, Yale University

Primary-stimulus generalization has been investigated in numerous experiments (1, 6, 7, 8, 13) but little research has been applied to drive-stimulus generalization. Moreover, experimental data directly concerning its quantitative characteristics are not available. Indirect evidence bearing on this problem has been derived, however, in investigations primarily concerned with other phenomena.

Heathers and Arakelian (4) found that animals trained on a Skinner box bar-pressing response and extinguished twice at the same degree of hunger resisted extinction less than animals extinguished once at each of two different hunger levels. This suggests that the effects of extinction are to a certain degree specific to the intensity of the drive stimulus operating during extinction. The motivation curves obtained by Heron and Skinner (5) and Perin (14) tend to indicate that stimulus-intensity generalization also operates in the case of reinforcement effects.

The concept of the primary generalization of drive seems to have been first put forward systematically by Hull, in 1943 (9). He postulated that the principle of stimulus-intensity generalization applies to S_D, citing the data then available in support of the hypothesis. According to Hull's concept, when the intensity of the drive involved in habit formation is changed, the original drive-stimulus component of habit strength ($_{s_D}H_R$) is changed to effective drive-stimulus habit strength ($_{s_D}\bar{H}_R$). The strength of $_{s_D}\bar{H}_R$ is a function of the difference between a given intensity of S_D and the S_D intensity involved in the original habit formation. Effective reaction potential ($_{s}\underline{E}_R$) will be determined by $_{s_D}\bar{H}_R$ and the drive intensity producing the $_{s_D}\bar{H}_R$.

An experiment by Webb (15), on the motivational effects of varying degrees of irrelevant drive strength, yielded results related to drive-stimulus generalization. The extinction data of this study indicate that response strength with an irrelevant, generalized drive state is less than that with a relevant, nongeneralized drive even when the former state can be assumed to be at least as strong in primary drive as the latter. The operation of drive-stimulus generalization is also evident in the study by Jenkins and Hanratty (11) on the cue value of different strengths of drive. These investigators found that changes in hours of food deprivation produced changes in response latency in a manner that indicated the presence of a drive-stimulus gradient.

On the basis of latent learning results, Meehl and MacCorquodale hypothesized the characteristics of such a gradient to be as follows (12, p. 392):

[1] The present study is the concluding portion of a thesis presented to the Faculty of the Graduate School of Yale University in partial fulfillment of the requirements for the degree of Doctor of Philosophy. The investigation also represents a part of the coordinated research program of the Institute of Human Relations. The author is greatly indebted to Professors Clark L. Hull and Mark A. May for their generous aid.

[2] Now at Indiana University.

"Essentially, we suggest that the steepness of the generalization gradient for *drive* as a stimulus is much greater on the low (weak drive) side of the point conditioned than it is on the high (strong drive) side. That is to say, when we increase the level of an animal's drive, while it is true that there is a loss from the standpoint of habit strength to generalized habit strength, the loss is not nearly as pronounced as it is when we move to a lower state of drive on the continuum."

The purpose of the investigation reported here was to secure direct quantitative evidence on the question of whether drives as such do in fact show intensity generalization. The particular drive used was that of hunger.

Subjects and Apparatus

The 195 male albino rats which served as *S*s in the present experiment were animals used in a previous study (17), and were approximately 11 months old.

The apparatus was the modified Skinner box which has been described in detail elsewhere (16). A shutter controlled the animal's access to the manipulandum and also provided a means of obtaining response latencies. The response consisted of a leftward movement of a horizontally protruding manipulandum. Humidified food pellets with a dry weight of approximately 0.1 gm. were used as reinforcement.

Procedure

Five groups of animals, each at a different degree of food deprivation, were trained to the limit of learning on the manipulandum response before S_D tests were made. The various training hours of hunger (h), with the number of *S*s in each group, were: 3h (43), 12h (39), 24h (38), 48h (38), and 72h (37). Each group was tested at three degrees of food deprivation not experienced during the training trials. No tests were made at 12h because diurnal complications would necessarily have been produced. The testing hunger interval at 24 hr. was arbitrarily omitted for the 12h group to reduce the four possible nontraining hunger intervals to three.

The test trials occurred between training trials 72–73, 76–77, and 80–81; each animal was given one reinforced test trial on each of these occasions. As previously indicated (17, p. 109), all groups were kept on a 5-day maintenance cycle consisting of 2 days of feeding followed by 3 days of food deprivation. Training sessions occurred at the appropriate time within the food deprivation period, four training trials being given per session. On the generalization test sessions, however, only one test trial was given during each session. Therefore, test trials were separated by four trials at the hunger level of the original training.

The test trials were arranged in a counterbalanced order. With each group being tested at three different hours, six permutational orders were necessary as a control for sequence of testing. For this reason each hunger group was divided into six subgroups, the training latencies obtained on trial 69 being used to rank the subjects within each group. Trial 69 was the first trial of the training session given prior to the beginning of testing. Groups of six animals were taken in rank order of latency and the rats of each of these groups were randomly assigned to the various sequence subgroups. Where the number of animals in a hunger group was not a multiple of six, the stratification procedure was arranged so that the rats unassignable to a stratum were those in the mid-range of rank order. These animals were also randomly assigned to subgroups.

In the classical stimulus-intensity generalization experiment, a stimulus of a given intensity is connected with a response and a test is then applied to this and to numerous other intensities of the same stimulus. This procedure introduces into the experimental results two parallel causal factors: (1) the factor of primary stimulus generalization, and (2) the factor of stimulus-intensity dynamism (10); this leaves an uncertainty as to what extent, if any, the results are caused by each factor. The experimental procedure employed in the present study, however, permits the testing of a response connected with a number of different drive-stimulus (S_D) intensities at a *single* intensity value. This limits to generalization alone, *at any given test intensity value*, the causal effect obtained by eliminating *in the test situation* the factor of *stimulus-intensity variation*.

Other procedural differences between the present experiment and the investigations, for example, of Hovland (6, 7, 8) and Brown (2), are that the test trials were reinforced and that only one test was made with each nontraining S_D.

Results

The mean and median response latencies obtained at the training and generalization test hours of hunger are shown in Table 1. The figures in bold-face type are the latency values

TABLE 1

MEAN AND MEDIAN RESPONSE LATENCIES IN SECONDS FOR THE VARIOUS HUNGER DRIVE GROUPS AT THE ORIGINAL AND TEST HOURS OF HUNGER *

Hours of Hunger During Testing		Hours of Hunger During Training				
		3	12	24	48	72
3	Mean	1.065	1.387	2.327	10.226	17.285
3	Median	.969	1.187	1.054	1.188	1.646
24	Mean	1.101		.992	1.108	1.118
24	Median	.932		.846	.904	.962
48	Mean	1.030	1.195	.980	.993	1.102
48	Median	.883	.980	.886	.871	.950
72	Mean	1.237	1.163	1.166	1.192	.984
72	Median	.974	.913	.826	.890	.848

* The nongeneralized latencies are shown in bold-faced type and were obtained at trial 69.

of each group at the training hours of hunger on trial 69. Since the latencies had already become asymptotic (17), these values are presumably representative of the training latencies for the various hunger groups during the testing period.

The latencies were converted into ${}_sE_R$ units by a previously determined equation (16) giving the relationship of absolute reaction potential (${}_s\dot{E}_R$) to median reaction latency (${}_st_R$). This equation is:

$$ {}_s\dot{E}_R = 6.26 {}_st_R^{-.363}. $$

To find the mean reaction potential values, the procedure for determining momentary reaction potential (18) was applied, the latencies obtained by individual rats being transformed into reaction potential values. The mean ${}_sE_R$'s for the various test points are presented in Table 2. Because the reaction potentiality is generalized, the ${}_sE_R$ values are designated as *effective* reaction potential (${}_s\underline{E}_R$).

It was previously reported (17) that significant differences between the latencies of the different drive groups were not obtained at the limit of learning. This was not the case with number of reactions to extinction. This discrepancy indicates that the asymptotic nongeneralized latencies are somewhat insensitive measures of differences in drive intensity. This suggests that some distortion may also be inherent in any consideration of the generalized latencies obtained at different drive intensities through changes in drive intensity. However, *when drive is constant*, latency is a fairly sensitive indicator of reaction potential (3). Therefore in analyzing the results of this experiment, the basic comparisons were made between data obtained under identical drive intensities. In terms of Tables 1 and 2 comparisons were made between the values appearing within a row.

Evidence of drive-stimulus generalization is indicated in the data shown in Table 2 by the fact that 14 of the 15 generalized reaction potential (${}_s\underline{E}_R$) values are lower than their respective nongeneralized reaction potential (${}_sE_R$) values. With the hypothesis of a chance occurrence of ${}_s\underline{E}_R$ values above and below the ${}_sE_R$ values, the probability that at all S_D test levels only one ${}_s\underline{E}_R$ value would fall above its reference ${}_sE_R$ value is $16/2^{15}$, or 5 times out of 10,000. Additional

TABLE 2

TABLE OF MEAN REACTION POTENTIAL VALUES *

Hours of Hunger During Testing	Hours of Hunger During Training				
	3	12	24	48	72
3	**6.356**	5.728	5.850	5.516	4.610
24	6.358		**6.536**	6.401	6.220
48	6.435	6.289	6.538	**6.489**	6.249
72	6.196	6.236	6.321	6.380	**6.528**

* The response latencies of individual Ss were converted into reaction potential values by the previously determined (16) equation:

$$ {}_s\dot{E}_R = 6.26 \; {}_st_R^{-.363}. $$

Means were computed for the various generalized reaction potential (${}_s\underline{E}_R$) values and nongeneralized reaction potential (${}_sE_R$) values. The latter are shown in bold-faced type and were obtained from the latency data at training trial 69.

tests of the significance of differences in $s\underline{E}_R$ were made at each hour of hunger used in testing for generalization. A two-component analysis of variance for the 24-, 48-, and 72-hr. testings did not produce significant F's.

An analysis of variance of the data at 3h was not possible because of a significant lack of homogeneity of variance. Therefore separate chi-square tests were made of the various paired training-hour groups. The median $s\underline{E}_R$ value of the pooled data of each pair of groups was found and frequencies of cases above and below this midpoint were determined for each group. Significant chi squares were obtained for the following paired groups tested at 3 hours: 12h vs. 3h (<1%); 72h vs. 3h (<1%); and 72h vs. 24h (2%).

Rows 1 and 4 of Table 2 showing the $s\underline{E}_R$'s obtained at 3 and 72 hr., respectively, are graphically presented in Fig. 1. It can be seen that $s\underline{E}_R$ is linearly related to hours of hunger during training. An inspection of the rows 2 and 3 of Table 2 indicates similar linear relationships. The least-squares fits obtained at the various test food deprivation intervals are presented below, with the range of application indicated in parentheses.

$$3h: \quad s\underline{E}_R = 6.289 - .0213h \quad (3h\text{–}72h)$$
$$24h: \quad s\underline{E}_R = 6.332 + .0085h \quad (3h\text{–}24h)$$
$$s\underline{E}_R = 6.703 - .0066h \quad (24h\text{–}72h)$$
$$48h: \quad s\underline{E}_R = 6.379 + .0027h \quad (3h\text{–}48h)$$
$$s\underline{E}_R = 6.969 - .0100h \quad (48h\text{–}72h)$$
$$72h: \quad s\underline{E}_R = 6.186 + .0046h \quad (3h\text{–}72h)$$

Another characteristic of the two $s\underline{E}_R$ gradients in Fig. 1 is the difference in the absolute values of their slopes. This indicates that less $s\underline{E}_R$ is generalized when the direction of change in S_D is from a high value to a low one than when the change is from low to high. The difference between the slopes, disregarding sign, of the 3- and 72-hour $s\underline{E}_R$ gradients is significant at the 1.2% level. The t of the difference is 3.55 with 6 df.

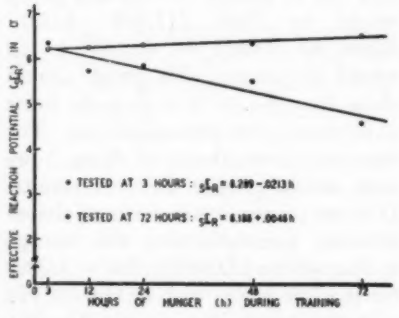
EFFECTIVE REACTION POTENTIAL ($_{S}\underline{E}_R$) IN σ
HOURS OF HUNGER (h) DURING TRAINING
• TESTED AT 3 HOURS: $_{S}\underline{E}_R$ = 6.289 − .0213h
• TESTED AT 72 HOURS: $_{S}\underline{E}_R$ = 6.186 + .0046h

FIG. 1. A graphic representation of the gradients of effective reaction potential ($s\underline{E}_R$) obtained by testing the various hunger groups at 3 and 72 hr., based on values presented in Table 2

DISCUSSION

The difference in slopes of the drive-stumulus generalization gradients implies that if a group of subjects is conditioned at the low extreme of a given intensity range, and if another group is conditioned at the high extreme, the difference between the reaction potentials of the two groups, when both are tested at the high extreme, will be less than when they are tested at the low extreme.[3]

The results of Hovland's experiment on sound-stimulus generalization (7) corroborate this hypothesis. Hovland found that the group conditioned at 40 decibels' intensity produced a mean galvanic skin response amplitude of 11.30 at 40 decibels, and a mean amplitude of 15.75 at 86 decibels; for the group conditioned at 86 decibels'

[3] A specific comparison of reaction potential differences can be made with the 3- and 72-hr. groups from the appropriate values in Table 2: 6.528–6.196 vs. 6.356–4.610 or .332 vs. 1.746.

intensity, the mean amplitudes were 8.90 at 40 decibels and 17.35 at 86 decibels. The difference in amplitudes at the low extreme, therefore, was 2.40, whereas the difference at the high extreme was 1.60.

Opposite results, however, were obtained by Brown (2) in an experiment testing the adient pull of rats to food associated with screens of .02, 5, and 5000 apparent foot-candles illumination. The difference in mean adient pull was considerably greater when the two groups conditioned at either extreme of illumination were tested at 5000 apparent foot-candles than when they were tested at .02 apparent foot-candles.

Hull's theoretical quantitative analysis of stimulus-intensity dynamism and generalization (10, p. 72) also conflicts with the results of the present study. The difference between the nongeneralized and generalized reaction potentials at the low extreme of the range considered was less than at the high extreme. The theoretical consideration of the joint action of stimulus-intensity generalization and stimulus-intensity dynamism yields one basic similarity, however, to the results of the present study. This similarity is embodied in Corollary IV (10, p. 73):

> "The general trend of the joint ${}_{s}E_{R}$ gradient originating at a weak stimulus intensity and generalizing toward stronger stimulus intensities has a smaller tendency to a downward slope than that extending in the opposite direction between the same stimulus intensities."

These similarities and contrasts with other studies suggest the possibility that differences in both stimulus modality and type of response may be important factors in producing certain divergent results in generalization experiments. An additional factor may be present, since the results of the present study seem to indicate that drive-stimulus generalization gradients are symmetrical in some regions of an intensity range and asymmetrical in others. The slopes of the two gradients of ${}_{s}\underline{E}_{R}$ at $24h$, for example, are approximately equal.

In general, however, it has been found that for a given direction of change the greater the difference between the conditioned and the test stimuli, the less will be the generalized or effective reaction potential. This relationship provides a basis for testing the two-component analysis of drive set forth in a previous article (17) by the present author. According to this hypothesis, effective drive ($\bar{D}$) is produced by the joint effects of drive proper (D) and inanition (ϵ). Although $\bar{D}$ decreases after approximately 60 hr. of food deprivation, D theoretically continues to increase as a positive monotonic function of hours of hunger. This means that $\bar{D}$ is less at 72 than at 48 hr. (11.207 vs. 11.895), whereas D is greater at 72 than at 48 hr. (19.700 vs. 14.115). If $3h$ is used as a test point, the change in $\bar{D}$ for the $72h$ group would be 7.207 (11.207 − 4.000) and the $\bar{D}$ change for the $48h$ group would be 7.895 (11.895 − 4.000). Since the change for the $48h$ group would be greater, this group should show less ${}_{s}\underline{E}_{R}$ at $3h$ if $\bar{D}$ is the basis of drive-stimulus generalization. The two-component theory of drive, however, would predict different results. If drive proper is the basis of drive-stimulus generalization, the change to $3h$ would be 15.699 (19.700 − 4.001) for the $72h$ group, and 10.114 (14.115 − 4.001) for the $48h$ group. The $48h$ group, therefore, should have more ${}_{s}\underline{E}_{R}$.

The results obtained when the 48-

and 72-hr. groups were tested at 3*h* are shown in Table 2 and Fig. 1. There it may be seen that the ${}_{s}\underline{E}_{R}$ value of the 48-hr. group is greater. This is also the case at 24*h*. The difference in ${}_{s}\underline{E}_{R}$ of the 48- and 72-hr. groups at 3*h* is significant at the 2% level with a *t* of 2.30 for 73 *df*. Therefore, drive proper (*D*), one of the two components of $\bar{D}$, is presumably the main variable in S_D generalization.

Summary

In an experiment designed to determine some of the quantitative aspects of drive-stimulus intensity generalization, five groups of rats totaling 195 *S*s were trained at each of five different hours of food deprivation to operate a Skinner-box manipulandum. Each group was then given one reinforced test trial at 3 nontraining hours of hunger. The training hours of hunger were 3, 12, 24, 48, and 72; the test hours of hunger were 3, 24, 48, and 72. The results, based on effective reaction potential derived from response latencies, suggest the following implications:

1. The principle of stimulus-intensity generalization clearly applies to S_D.
2. The gradients of effective reaction potential at a given level of food deprivation are linear functions of the hours of hunger during training.
3. The slopes of the ${}_{s}\underline{E}_{R}$ gradients at 3 and 72 hr. are significantly different. These slopes indicate that less ${}_{s}\underline{E}_{R}$ is generalized when the degree of food deprivation is changed from high to low than when it is changed in the opposite direction.
4. The evidence supports the quantitative nature of the drive component of effective drive.

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PRE-RECALL WARMING-UP IN VERBAL RETENTION[1]

MARTY R. ROCKWAY AND CARL P. DUNCAN
Northwestern Univeristy

Ammons (1), in his theoretical analysis of motor learning, has suggested that much of the decrement in proficiency following a rest after practice on a motor learning task is due to a loss of "set." This loss, which he feels consists principally of favorable postural adjustments, he symbolizes D_{wu} and defines as "the decrement on any trial due to the necessity for the subject to 'warm-up' after rest" (1, p. 270).

Taking a lead from this and other studies (3, 9), Irion (5, 6, 7) attempted to relate the warming-up phenomenon to the retention of verbal material. He identified the warming-up effect with "the greater slope of the initial segment of the relearning curve relative to the slope of the original learning curve at a corresponding level of initial proficiency" (5, p. 338). Irion reasoned that if part of the decrement in retention following a rest interval was the result of a loss of set to perform, then any activity which would serve to maintain, or reinstate, the set present during original learning (OL) should decrease the warming-up effect and increase the amount retained. To test this hypothesis he designed an experiment (6) in which one of three groups of *S*s participated in a set-reinstating (warming-up) activity just prior to resuming practice on a list of paired adjectives after a 24-hr. rest interval. One trial of color naming served as the warming-up activity. The colors were arranged as a paired-associate list and were presented on the memory drum at the same anticipation rate as the adjectives. The *S*s were not instructed to learn the colors, but merely to call out the second color of each pair *after* it was visible in the window of the drum.

The results of this study indicated that one trial of color naming had effected an almost complete recovery from the retention loss which normally occurs during a rest interval of this length. In terms of performance on the initial trial of relearning (RL), the group which named colors after a 24-hr. rest interval was not significantly different from the group which received no rest between OL and RL. On the other hand, the performance of the color-naming group was significantly greater than that of the group which did not engage in a set-reinstating activity prior to RL after a 24-hr. rest interval.

Irion (5, p. 340) made the following predictions concerning the effect of warming-up on retention:

> 1. Retention should be greater following the performance during the rest period of a set-maintaining activity than following the performance of a set-disturbing activity. In the latter case, amount of retention loss should be an inverse function of the similarity between the rest period activity and the activity to be retained.
>
> 2. Regardless of the activities which occur during the rest period, if, immediately prior to measuring retention, *S* engages in an activity which reinstates an appropriate set to perform, retention should be greater than if this set had not been reinstated. In this case, the extent of recovery from retention loss should be a function of the similarity between the set-inducing task and the learning task, and of the amount of time devoted to the induction of appropriate set.

[1] The assistance of Dr. B. J. Underwood, Department of Psychology, Northwestern University, is gratefully acknowledged.

In further support of the second of these predictions, Irion and Wham (7) have recently shown that performance on the first RL trial of a serial list of nonsense syllables is an increasing function of the amount of pre-recall warming-up. The warming-up activity was a number recitation task which was presented on a memory drum in the same manner as the nonsense syllables.

The present paper is a report of two experiments which resemble most closely Irion's first study (6). The first is concerned with the effect of varying both the amount and the rate of presentation of a pre-recall warming-up activity upon the retention of verbal paired associates. The second is an attempt to determine whether the *S*'s knowledge, or lack of knowledge, of the purpose of a pre-recall warming-up activity affects retention.

Experiment I

This experiment was designed to test Irion's (5) predictions concerning the relationships between the warming-up task and the retention of a previously learned verbal list. According to these predictions it should follow that: (*a*) Varying the rate of presentation of the set-reinstating activity (color naming) should alter the similarity between it and the activity to be recalled, thereby effecting a change in the amount retained; i.e., the greater the degree of similarity between the warming-up task and the learning task the smaller should be the retention loss. (*b*) Varying the amount of time devoted to inducing a set by increasing the number of colors presented should result in a progressive reduction in the amount forgotten.

Procedure

Apparatus and materials.—The learning task consisted of the same 15-unit list of two-syllable paired adjectives employed by Irion (6). The list was typed in lower-case letters on glazed cloth tape and was presented on a Hull-type memory drum. This drum was mounted in a large fiberboard screen designed to conceal *E* from *S* and to reduce the number of distracting stimuli which might influence *S*'s performance. The list of words was presented at a 2-sec. anticipation rate; i.e., each stimulus word was first exposed for 2 sec., then the shutter lifted and both stimulus and response words were shown for an additional 2 sec. A 4-sec. rest interval was allowed between successive presentations of the list. The words were arranged in three different orders to minimize serial position effects.

The warming-up task consisted of strips of oil-colored adhesive tape arranged as a 30-unit paired-associate list. The strips were $\frac{3}{4}$ in. long and $\frac{1}{4}$ in. wide, so they occupied about the same amount of space as the average adjective. Eight different colors were used, and these were paired so that no two were in the same stimulus-response relationship more than once in the list.

As in Irion's study, *S*s in this investigation were required to read cartoons (2) for 5 min. following the completion of OL and for 5 min. prior to the first trial of RL. The *S*s performed this activity while seated in front of the memory drum.

Subjects.—Sixteen different *S*s were utilized in each of the 10 conditions. All *S*s were enrolled in the introductory psychology course at Northwestern University. Students in this course are required to serve a minimum number of experimental hours to secure class credit. None had had previous experience with verbal learning by memory drum presentation. Each *S* was required to be present at two experimental sessions 24 hr. apart. Approximately the first 90 *S*s were assigned to their respective experimental groups by means of a table of random numbers. Thereafter, *S*s were assigned in such a manner as to equate the means and variabilities of the groups on the basis of the number of correct anticipations made on the tenth trial of OL. An attempt was also made to maintain approximately equal numbers of men and women in all conditions. The selection of *S*s for a control condition did not begin until a preliminary analysis of the experimental data (12 *S*s per group) disclosed that it was unlikely that any statistically significant differences among experimental conditions would be forthcoming even with a larger *N*. Thereafter, *S*s were divided among the experimental groups and the control group so that the mean and variability of the tenth OL trial of the control group conformed to those of the experimental groups.

*S*s were eliminated from the experiment, (*a*)

if, by the end of the fifth OL trial, they failed to make a single correct response other than the demonstration word; (*b*) if, by the end of the fifth OL trial, they had made a perfect score on one of the first five trials; (*c*) if they admitted to deliberate rehearsal during the 24-hr. retention interval.

A total of 11 *Ss* were eliminated because of failure to meet the minimum criterion, while none were discarded for exceeding the maximum. Six *Ss* admitted to deliberate rehearsal, so their records were not included in the data.

Conditions.—Both the number of pairs of colors and the rate of presentation of the colors in the warming-up list were independently varied. Three lengths and three rates of presentation of the warming-up list were used (see Table 1). There were nine experimental conditions. A tenth (control) condition was included in which *Ss* did not engage in color naming. No pre-training or practice trials were given and no *S* served in more than one condition.

The warming-up list was attached to the right half of the same glazed tape on which the words were typed. The colors were arranged so that the blank space following the last pair in the warming-up list coincided with the blank space preceding the first pair of adjectives in the learning list. To switch from colors to words a slide was moved so as to expose the left window and cover the right window of the drum. In order to minimize the time required to transfer from colors to words, *Ss* in other than the 30-color groups were presented either the last 5 or the last 15 pairs of colors in the warming-up list. Thus all *Ss* in the experimental groups ended with the same pair of colors, and it took only a few seconds to go from colors to words.

On the first day all *Ss* received the same treatment. First, instructions for paired-associate learning were read, and then ten trials on the list of paired adjectives were given. Immediately thereafter, *Ss* read cartoons for approximately 5 min. Before being dismissed all *Ss* were read instructions cautioning against rehearsal during the 24-hr. rest interval.

On the second day *Ss* began the session with 5 min. of cartoon reading. Those in the experimental groups were then read instructions for color naming and RL. Upon completion of the appropriate warming-up task, all *Ss* received an additional ten trials with the paired adjectives.

In summary, the design for the nine experimental groups is as follows: 10 trials OL on the adjective list, read cartoons 5 min., 24-hr. rest, read cartoons 5 min., warming-up list, 10 trials RL on the adjective list. The procedure for the control group was identical except for omission of the warming-up task.

On each trial the *S*'s score was the number of responses anticipated correctly before the shutter was raised.

TABLE 1

CONDITIONS OF EXPERIMENT I

Condition	Number of Pairs of Colors in Warming-up List	Presentation Rate in Seconds
I	5	1
II	15	1
III	30	1
IV	5	2
V	15	2
VI	30	2
VII	5	4
VIII	15	4
IX	30	4
X (Control)	—	—

Results

This investigation was designed to permit an unconfounded evaluation of the effects of two independent variations of the warming-up activity upon the retention of verbal paired associates. To this end, the nine experimental conditions were selected so that the resulting data would be amenable to a replicated, two-classification analysis of covariance.

Analysis of the nine experimental conditions.—The first covariance analysis of the experimental conditions utilized the number of correct responses on the first RL trial (Trial 11) as the measure of retention. The correlated measure of initial proficiency employed was the number of correct responses on the last trial of OL (Trial 10). The means for Trials 10 and 11 are shown in Table 2, and a breakdown of sources of variance and the analysis of adjusted mean squares is presented in Table 3. It can be seen from Table 3 that neither the differences between the means for rates of presentation of the colors, nor the differences between the means for number of pairs of colors, is

TABLE 2

MEAN NUMBER OF CORRECT RESPONSES ON CRITICAL TRIALS IN EXP. I

Cond.	Trial 10	Trial 11	Trials 1–10	Trial 11 (adjusted)*
I	7.38	5.25	40.31	5.12
II	7.38	6.19	40.50	6.04
III	7.38	5.19	37.75	5.38
IV	7.38	6.38	37.81	6.56
V	7.31	6.25	41.94	5.92
VI	7.19	5.81	38.25	5.94
VII	7.44	5.69	39.00	5.72
VIII	7.38	5.37	40.50	5.27
IX	7.38	5.44	37.62	5.52
X	7.38	5.50	35.06	6.02

* Adjusted by means of the average regression of Trial 11 on Trials 1–10 of the nine warming-up conditions.

statistically significant. The interaction of pairs with rates is also not statistically significant.

When the *F*-tests employing the correlation between performance on the matching trial and the recall trial proved to be insignificant, the analysis was repeated using number of correct responses during Trials 1–10 of OL. Again, none of the *F*'s obtained was significant at the 5% level (Table 3).

Covariance was used in the first analysis (Trial 10 and Trial 11) primarily to secure a more precise estimate of error; i.e., the groups were matched on Trial 10 performance, thus they were almost identical with reference to this measure. However, in the second analysis (Trials 1–10 and Trial 11), the performance of the various groups on the first ten OL trials differed somewhat. In this case, therefore, it was possible to make adjustments in the means for the recall trial to compensate for the initial differences between groups on the ten OL trials. These adjusted means were calculated in accordance with the procedure outlined by Snedecor (**8**, p. 321 ff.). This technique employs the average regression of recall performance on OL performance to predict what the mean number of correct responses on the recall trial for each group would have been if all groups had been equally proficient on the ten OL trials. These adjusted means are given in Table 2. It seems apparent merely from visual inspection of these means that the adjustments effected no changes great enough to produce significant differences.

After both of the preceding analyses had failed to reveal significant differences between the experimental groups another attempt was made. This time the means for the total number of correct responses during the ten RL trials were subjected to an analysis of convariance. The total correct responses during OL were once again used as initial measures. The results of this analysis, which are not presented here, also failed to indicate any statistically significant differences among the experimental groups.

In addition to reducing retention loss, prerecall warming-up should also reduce the warming-up effect, i.e., the sharp initial rise in the RL curve. The change in proficiency between the first two RL trials may be used as a measure of the initial slope of the RL function. An examination of these data, not presented here, also failed to disclose any significant or systematic trend differences among the nine experimental groups.

The control condition vs. the experimental conditions.—As was indicated previously, the lack of significant

TABLE 3

ANALYSIS OF COVARIANCE OF THE NUMBER OF CORRECT RESPONSES ON TRIAL 11 FOR WARMING-UP CONDITIONS OF EXP. I

Source	*df*	Initial Proficiency Trial 10		Initial Proficiency Trials 1–10	
		Adjusted Mean Square	*F*	Adjusted Mean Square	*F*
Presentation rates	2	7.36	1.48[a]	6.14	2.54[a]
Pairs of Colors	2	2.31		0.28	
Interaction	4	2.00		3.25	1.34[b]
Within groups	134	4.97		2.42	
Total	142				

[a] *F* at the 5% level = 3.07 for 2 and 134 *df*.
[b] *F* at the 5% level = 2.44 for 4 and 134 *df*.

differences among the nine experimental conditions was not completely unexpected. If fact, it was to meet this contingency that a control group, which received no pre-recall warming-up, was included in the experiment. Because of the level of performance of the members of the control group, it seemed unnecessary to subject their data to precise statistical comparisons with the experimental groups. For instance, in Table 2, when the mean numbers of correct responses for Trial 11 are inspected it is evident that the mean for the control condition (5.50) is approximately equal to the grand mean of the nine experimental conditions (5.72). Since the mean performance of the control group on Trial 10 is matched with that of the warming-up groups, it is obvious that the inclusion of the control group in an analysis of covariance similar to the first one (Trial 10 and Trial 11) would not have resulted in a significant *F*-ratio among conditions.

The mean for Trial 11 of the control group was adjusted by using the average regression of Trial 11 on the ten OL trials of the nine experimental groups.[2] When the adjusted means for all conditions, presented in Table 2, are compared, it can be seen that the mean of the control condition, if introduced into a second analysis of covariance, would not have altered any conclusions drawn from the analysis as it stands.

A comparison of the difference in proficiency between the first and second RL trials of the control group with the same data from the warming-up groups did not reveal any significant trend differences. An inspection of the plotted RL curves for all ten groups, not shown here, also failed to suggest any significant slope differences.

[2] It was assumed that the regression of the control group is homogeneous with the regressions of the experimental groups.

Discussion

In view of the results from the analyses of the retention data of the experimental conditions, we are led to conclude that, in this particular investigation, the predictions that verbal retention is directly related to the similarity between the warming-up and learning tasks and to the amount of pre-recall warming-up were not verified. In fact, subsequent comparisons of the warming-up conditions with the control condition indicated that we also failed to obtain a significant gain in the amount retained by the groups subjected to warming-up prior to recall over the group which received no warming-up.

The failure of the groups which named colors at other than a 2-sec. anticipation rate to achieve a warmed-up state is not surprising, for, in terms of exposure interval, their warming-up task was dissimilar to the task to be recalled. On the other hand, an attempt should be made to account for the apparent lack of warming-up induced by the 2-sec. conditions, especially Cond. V, which is similar to Irion's (6) single trial of color naming.

Despite the fact that the present study was designed to duplicate as closely as possible that of Irion, there are some methodological disparities between the two investigations. These unintentional differences occurred because, at that time, Irion's exact procedure was not available to us. However, since discrepancies were present we shall point them out and attempt to evaluate their possible effects on the results of the two studies.

Insofar as they are known, the major differences between Irion's study (6) and ours are as follows, where each item listed is a procedural detail present in this experiment but not in Irion's.

1. When *Ss* received instructions for OL, a

pair of words from the list to be learned was used for demonstration purposes. Since no practice trials were given, this pair, which was almost never missed, provided a means of determining within the first few trials of OL whether or not an *S* had understood the instructions.

2. Before leaving the laboratory on the first experimental day, all *S*s received instructions cautioning against rehearsal.

3. When the *S*s were read the instructions for color naming and RL, they were *not* informed that the purpose of color naming was to aid them in getting set for the learning task.

Concerning the first point, using a pair of words from the list for demonstration purposes apparently introduced no important complicating factors into the present study. A comparison was made between the level of performance on each of the learning trials of the control group (24-hr. rest without warming-up) of this study and the similar group (Cond. B) in Irion's (6, p. 671) study. This comparison revealed that the seeming superiority of *S*s in the present investigation could be almost completely accounted for in terms of the additional contribution of the demonstration pair.

Secondly, since both Irion's control group and his warming-up group did not receive anti-rehearsal instructions after OL, it seems unlikely that the superior performance of his warming-up group on the recall trial (Trial 11) was due to rehearsal.

As for the third difference, perhaps telling *S* that color naming will get him back into the "swing" of learning adjectives affects the way in which he performs the warming-up task. It may be that the *S* who feels that color naming serves a useful purpose participates more vigorously in this activity and, in this way, more effectively reinstates a set.

Experiment II

In view of the negative results obtained in Exp. I, it was felt that another effort should be made to duplicate the findings of Irion. This time it was thought advisable to copy exactly Irion's materials and procedure, and, in addition, to include a condition in which *S*s are not told the purpose of the color naming. It seems improbable that instructions are such a powerful factor that *any* task given prior to recall would reduce retention loss if the *S* were instructed that this was its purpose. However, it is possible that an otherwise adequate warming-up task requires positive instructions to prove effective in certain instances.

With the exception of the instructional difference deliberately introduced into Cond. B (see below), the conditions of Exp. II reproduce, as closely as possible, Irion's (6) Cond. B (24-hr. rest without warming-up) and Cond. C (24-hr. rest with one warming-up trial before recall).

Procedure

Apparatus and materials.—The learning task was the same list of paired adjectives used in Exp. I. The method of presentation of the list was similar to that described previously, except that the intertrial rest interval was lengthened to 8 sec.

The warming-up task was again color naming. Six colors were arranged as a list of 15 paired associates and presented at the same 2-sec. anticipation rate as the adjective list.

All *S*s were required to read cartoons for 5 min. subsequent to OL and for 5 min. prior to the first trial of RL. While performing this activity, *S*s were seated in front of the memory drum.

Subjects.—Sixty *S*s, 20 in each of the three conditions, were used. All *S*s were enrolled in the introductory psychology course at Northwestern University. None had had previous experience with verbal learning by memory drum presentation. The *S*s were assigned to one of the three conditions in the order of their appearance at the laboratory for the first experimental session.

*S*s were eliminated from the experiment on the basis of the first two criteria used in Exp. I (p. 307). Six *S*s were eliminated for failure to meet the minimum criterion, while none was discarded for exceeding the maximum.

Conditions.—In Cond. A, *S*s received instructions for color naming which included statements (quoted below) telling them the purpose of this activity. The *S*s in Cond. B received instructions for color naming which did *not* tell them the purpose of this activity. Condition C was a control group which did not name colors.

On the first day all *S*s received the same treatment. First, Irion's (6) instructions for paired-associate learning were read, and then ten trials on the list of paired adjectives were given. Following this, *S*s read cartoons for approximately 5 min. before being dismissed for 24 hr. No anti-rehearsal instructions were given.

On the second day all *Ss* began the session with 5 min. of cartoon reading. Then *Ss* in Cond. A and B were read appropriate instructions for color naming and RL. Included in the instructions given to *Ss* in Cond. A, but not to those in Cond. B, was the following information: "The purpose of the color naming is to get you back into the swing of the learning task. While you are naming the colors, try to regain your judgment of how fast the machine runs, of how you can do your best on this task." These two groups then engaged in one trial of color naming followed by ten RL trials with the paired adjectives. The *Ss* in Cond. C did not name colors prior to RL. Instead, following the cartoon reading, they were read instructions for RL and received ten additional trials with the paired adjectives.

Results

The means for Cond. A, B, and C for the last trial of OL (Trial 10) were 6.95, 6.85, and 6.95, respectively. The corresponding means for the first trial of RL (Trial 11) were 4.95, 5.50, and 4.90. The recall scores (Trial 11) for the three conditions were subjected to an analysis of covariance using the total number of correct anticipations during the ten OL trials as initial measures. As can be seen from the summary of this analysis in Table 4, the differences among the recall means for the three conditions were not significant.

Using the total number of correct responses during OL as initial measures, a covariance analysis of the means for the total number of correct responses during RL was made. The results of this analysis, not presented here, did not produce any significant *F*-ratios.

An inspection of the plotted RL curves and the differences in proficiency between the first two RL trials of the three groups failed to reveal any significant differences in the initial slopes of the RL curves.

TABLE 4

ANALYSIS OF COVARIANCE OF THE MEAN NUMBER OF CORRECT RESPONSES ON TRIAL 11 FOR EXP. II

Source	*df*	Adjusted Mean Square	*F*
Between groups	2	8.39	<1[a]
Within groups	56	25.47	
Total	58		

[a] *F* at the 5% level = 3.17 for 2 and 56 *df*.

Discussion

It seems necessary to conclude that, regardless of the kind of instructions given, naming colors immediately prior to the first RL trial did not produce a reduction in the forgetting of previously practiced paired adjectives, nor did it reduce the steepness of the initial slope of the RL curve.

We are unable to account for the differences in results between Irion's experiments and ours. Correspondence with Dr. Irion after each of our experiments had been completed revealed no differences between his procedure and ours other than those which were reported above and tested in Exp. II. It is possible that there were differences between populations of *Ss* or differences in interaction of *Es* with *Ss*. Further independent work, however, is needed to determine whether these or other factors account for the inconsistent results from experiments on warming-up.

SUMMARY

Two experiments were designed to study the effect of variations of a warming-up (color naming) task upon the retention of verbal paired associates.

In Exp. I, ten groups of *Ss* received ten trials of original learning (OL) on a list of 15 paired adjectives presented on a memory drum at a 2-sec. rate of presentation; 24-hr. later an additional ten trials of relearning (RL) were given to all groups. The total number of correct

responses on the first RL trial was used as the recall score, and the total number of correct responses on the ten RL trials was used as the RL score. Immediately prior to the first RL trial, nine of the groups named the second members of 5, 15, or 30 pairs of colors presented at a 1-, 2-, or 4-sec. anticipation rate on the same drum as the words. The tenth (control) group did not name colors before RL.

In Exp. II three new groups of *Ss* received ten OL trials on the same word list used in Exp. I and 24 hr. later received ten RL trials. Immediately prior to the first RL trial *Ss* in Cond. A and B engaged in one warming-up trial of color naming. The warming-up list consisted of 15 pairs of colors presented at the same rate as the words. The *Ss* in Cond. A were informed of the purpose of the color naming while those in Cond. B were not. The *Ss* in Cond. C (control) did not name colors before RL.

The results of both experiments showed no evidence for pre-recall warming-up. No explanation seemed available to account for the failure to find evidence for pre-recall warming-up in this investigation.

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THE ACQUISITION AND EXTINCTION OF CONDITIONED EYELID RESPONSES AS A FUNCTION OF THE PERCENTAGE OF FIXED-RATIO RANDOM REINFORCEMENT [1]

DAVID A. GRANT AND LOWELL M. SCHIPPER
University of Wisconsin

The purpose of this experiment was to determine the effect of five different percentages of fixed-ratio reinforcement upon the acquisition and extinction of the conditioned eyelid response.

The results of Humphreys' pioneer experiment (**11**) in this area are well known. He used 50% and 100% reinforcement and found essentially identical acquisition curves, but the extinction trends were vastly different. Partially reinforced *S*s maintained their CR's at a consistently higher level during extinction than did *S*s trained under the 100% schedule of reinforcement. A great number of studies dealing with partial reinforcement have been published in the last 12 years (**2, 4, 5, 13,** *et al.*). In these investigations results have been obtained on various conditioning procedures and on many types of organisms. It is notable that very few studies have investigated percentages of reinforcement other than 100% or 50%. Later investigations of eyelid conditioning under partial reinforcement (**4, 8**) have confirmed the more important aspects of Humphreys' findings but there have been differences in details. Some of these differences apparently can be attributed to the presence of β-responses (**4**) in the earlier data of Humphreys.

The present investigation might be thought of as a more complete dimensional analysis of the problem which Humphreys introduced. Aside from the general advantages of a dimensional analysis, the present study contains relatively pure eyelid conditioning because of the control of the sensitized β-responses. In addition, it has been pointed out by Grant, Hake, and Hornseth (**5**) that the particular percentages of reinforcement used in the original experiment, 100%, 50%, and 0% reinforcements, had special mathematical implications so that a more complete exploration of the function was highly desirable.

Apparatus and Procedure

Apparatus.—Except for the intensity of the conditioned stimulus (CS) and the instructions to *S*, the apparatus and general procedure of this experiment were precisely the same as that of Grant, Riopelle, and Hake (**8**). The CS was a light, and the UCS was a corneal air-puff. These were presented in standard time relationship by a Dodge pendulum photochronograph (**3**). The conditioned stimulus involved an abrupt change in brightness of a milk-glass screen from 14 millilamberts up to 220 millilamberts. The UCS was adjusted so as to produce an adequate corneal reflex. The *S*s were run under standard light-adapted conditions so as to prevent the occurrence of sensitized β-responses (**6, 7**).

Experimental design.—The plan of the experiment was quite simple. Each *S* was given a 60-trial training session on Day 1. On Day 2 he was given 32 trials of training followed immediately by 25 extinction trials. Five groups, of 14 men and women subjects each, were run in the present experiment. The only difference in the five procedures was in the percentage of reinforcement in the training trials. The groups

[1] This investigation was supported in part by the Research Committee of the Graduate School of the University of Wisconsin with funds voted by the State Legislature.

may be designated the 0%, 25%, 50%, 75%, and 100% groups. The 0% group received no reinforcement trials during the training period on Days 1 and 2. The 25% group received 25% reinforced trials in the training period with 75% nonreinforced trials randomly interspersed among these. The 50% group received 50% reinforced trials during the training period, etc. In setting up the random sequences of reinforced and unreinforced trials, the only departures from strictly random assignment were that the first and last training trials on Day 1 and Day 2 were always reinforced and the proportion of reinforced trials was maintained constant for successive eight-trial blocks.

Subjects.—The *Ss* were 70 men and women students who volunteered from the classes in elementary psychology at the University of Wisconsin. They were randomly assigned to the five groups in order of their appearance for the experiment. They received standard instructions to maintain a detached attitude and not to attempt to control voluntarily their natural reactions to the stimuli.

Results

In the present study, any eyelid closure, occurring within the latency range of 150 to 450 msec. after the onset of the CS, was defined as a CR. Selection of this range was based upon previous studies (**6, 7**) which have shown that under light-adapted conditions, most of the CR's and practically no β-responses, will occur within this range.

The essential findings of the experiment are shown in Fig. 1. In Fig. 1 the per cent frequency of CR's is plotted for successive blocks of trials throughout the experiment. The parameter is the per cent of reinforced trials during the training period. As indicated below the figure, the precise size of the block varies at different stages of the experiment. Examination of Fig. 1 reveals a number of interesting trends. The groups have become differentiated by the end of the first eight-trial block and are sharply differentiated throughout the training trials. In general, all of the

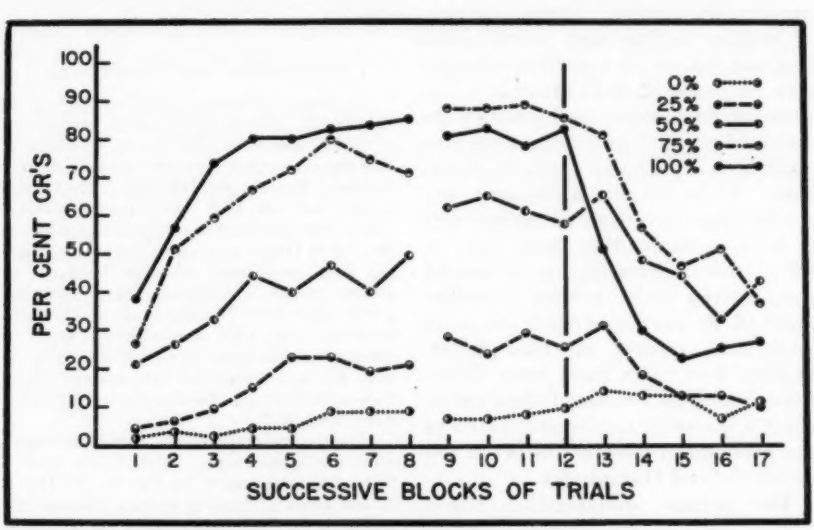

Fig. 1. The course of acquisition and extinction of the eyelid CR with varying percentages of reinforced trials during training. Per cent frequency of CR's is plotted against successive blocks of trials. The size of the block during training (1–12) is eight trials except for Block 8 which is four trials. The size of the block during extinction (13–17) is five trials.

TABLE 1

SUMMARY OF ALEXANDER TREND ANALYSES FOR ACQUISITION AND EXTINCTION CURVES OF FREQUENCY OF CR's

Source of Variation	Acquisition Trend		Extinction Trend	
	df	*F*	*df*	*F*
A. Between group means	3	443.576**	3	86.572**
B. Between group slopes	3	8.687**	3	2.650**
C. Group deviations from estimation	30	1.606*	9	0.678
D. Between individual means	52	32.033**	52	24.276**
E. Between individual slopes	52	5.726**	52	3.240**
F. Over-all slope	1	326.303**	1	102.297**
G. Over-all deviations from linearity	10	8.388**	3	11.072**
H. Individual deviations from estimation (error estimate)	520	*1.650*	156	*0.566*

* Significant at 5% level of confidence.
** Significant at 0.1% level of confidence.
The data analyzed in this table were the raw frequencies per successive eight-trial (except block eight on Day 1) blocks during training and successive five-trial blocks during extinction.

curves showed some rise, the greatest rises being found in the 100% and 75% group, with a lesser rise being found in the 50% group and progressively less in the 25% and 0% groups. During the Day 2 training trials, the 75% group responds at a higher frequency than the 100% group but otherwise the relationships established on Day 1 are maintained. There is no evidence of additional acquisition of CR tendencies on Day 2. Each group tends to respond at a fairly constant rate on Day 2 training trials. During extinction there is a sharp drop in per cent frequency in the 100% group, a lesser drop in the 75% group, a brief rise followed by a drop in the 50% and 25% groups, and a rather gradual rise to a stable level in the 0% group. The gradual rise in per cent frequency of responses in the 0% group is probably an artifact due to incomplete elimination of the β-responses. The extinction trends of the other groups are, in general, what would be expected from the previous literature on partial reinforcement.

In Table 1 is summarized an Alexander trend analysis (1) for the acquisition and extinction scores. Acquisition and extinction frequencies were analyzed separately as indicated in the table. The results of the 0% group were eliminated from the analysis because they would tend to inflate some of the *F*'s by virtue of the flat slope involved in this group. Thus, the analysis indicates the significance of differences between the 100%, 75%, 50%, and 25% groups. During acquisition, the differences between group means and group slopes, shown in lines A and B, were highly significant. There was some indication of a difference between the nonlinear components of the curves as shown by the *F* of 1.606 in line C. Wide differences between the individual means and slopes during both acquisition and extinction are indicated in lines D and E. When the *F*'s for lines D and E are compared with the *F*'s for lines A and B, respectively, it is seen that the difference between group means is significantly greater than could be accounted by individual differences, but the difference between group slopes is about what would be

predicted in terms of the differences found between individual slopes. Lines F and G indicate simply that there was significant upward trend, and that this trend was nonlinear, respectively. And in line H is given in italics the basic error estimate on which the *F*'s of the column were based.

A review of the extinction data shows that the same tendencies stand out. Differences were found between group means and between group slopes, but there were no significant differences in the nonlinear components of the extinction trends. The differences between individual means were highly significant, but the differences between group means remained significant when compared with the mean square between individual means. Again lines F and G show significant nonlinear decrement trends in the extinction data as should be expected. The results of the statistical analysis can be summarized by the statement that the obvious trends appearing in Fig. I are for the most part supported by the significance levels obtained.

A figure similar to Fig. 1 was prepared, plotting average magnitude against successive blocks of training and extinction trials. This figure has not been included here because trends similar to those of Fig. 1 were found except that the 75% group showed a higher average magnitude of CR's throughout the experiment. Also, the average magnitude of CR's in the 50% group was relatively higher than was the case in the frequency data, and the magnitude of CR's in the 25% group was relatively much lower than in the frequency data. It was felt that the per cent frequency data were more valid because the occurrence of the reflex to the puff during reinforced trials necessitated, in some instances, reading the CR magnitude somewhat lower than it might be read had not the puff reflex intervened. This artifactual reduction of magnitudes, as read, increased with increased reinforcement percentage.

The experiment was designed to produce a plot of the per cent of CR's as a function of the percentage of reinforced trials during training. This relationship can be expressed in various ways, and one such plot is presented in Fig. 2. Here the per cent of CR's at four phases of the experiment is plotted as a function of the per cent frequency of reinforced trials during training. The 0% data are omitted as not being comparable to the other data. The parameter is the stage of acquisition or extinction. The boldest line gives the per cent frequency of CR's at the end of the training period during the twelfth block of Fig. 1. Here the increased effectiveness of the higher percentages of reinforcement is readily apparent. The other three curves in Fig. 2 give the per cent frequency of CR's at three stages of extinction, during the

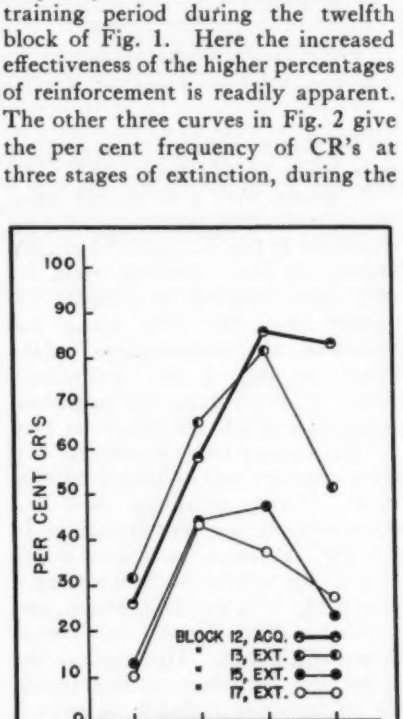

FIG. 2. The per cent frequency of CR's at the termination of training (Block 12) and at three stages of extinction as a function of the per cent frequency of reinforced trials during training

first five trials, during the middle five trials, and during the last five trials. It may be noted from Fig. 1 that during the first five trials of extinction the per cent frequency rises for the 0%, 25%, and 50% groups, drops slightly for the 75% group, and drops sharply for the 100% group. By the middle of the extinction process, all groups except the 0% group have dropped, with the drop being most pronounced in the 100% group and least pronounced in the 75% group. At the end of 25 extinction trials the drop has continued for all groups, except that there has been a slight upturn in the trend of the 100% group.

In discussing the implications of Fig. 2, it might first be remarked that during acquisition a similar plot could be made which would show a gradual increase in the per cent frequency of CR's in all of the groups with a most pronounced increase in the 75% and 100% groups until the ultimate performance would be that given by the heavy curve in Fig. 2. During extinction we see a rapid deterioration in the performance of the 100% group and a somewhat less rapid deterioration in performance of the other groups. In a general way, this would appear to indicate that there is a sort of hard core of resistance to extinction in the region between, say, 40% and 80% reinforcement ratios. Below about 30% reinforcement there is apparently not enough conditioning taking place during the training period, and above 80% a rapid extinction is obtained, so that the resulting high response level during extinction is obtained for the 50% and 75% groups in the present study.

This tendency for 50% and 75% reinforcement ratios to be most effective in producing resistance to extinction is clearly apparent in Fig. 3 where

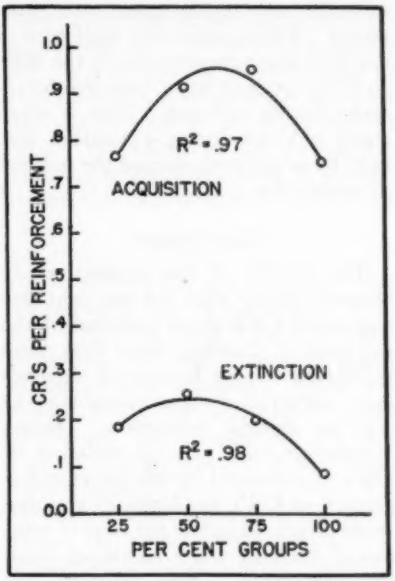

FIG. 3. The number of CR's per reinforced trial during acquisition and extinction. Quadratic curves have been fitted by means of orthogonal polynomials to the data points. The squares of the correlation coefficients between estimated and actual data values are given as R^2 for each curve (17, p. 392).

the number of CR's during extinction was divided by the number of preceding reinforcements and the result was plotted against the per cent of reinforced trials during training. It may be noted that whereas the 75% reinforcement procedure was shown in Fig. 1 and Fig. 2 to be most effective with respect to the total number of CR's produced during extinction, Fig. 3 shows that the 50% procedure produces more CR's per reinforcement during extinction than does any of the other reinforcement schedules. It will also be noted from the upper curve in Fig. 3 that the 75% procedure and the 50% procedure are superior to the 25% and 100% procedures during acquisition when the number

of CR's per reinforcement are compared. Thus, again, the hard core of conditioning appears to lie in the 50% to 75% range of fixed ratio reinforcement during training. This is especially obvious during extinction but may be noted even during the process of acquisition.

Discussion

The results of the present study showed clearly that the per cent frequency of CR's was a function of the per cent of training trials that were reinforced. This functional dependence occurred during acquisition as well as during extinction. During acquisition, the rate of emission of CR's as measured by the per cent frequency of CR's per block of training trials increased as the per cent of reinforced training trials increased from zero to 100%. The function appeared to be negatively accelerated, and it is possible that it had a maximum at less than 100% reinforced trials. During extinction, the rate of emission of CR's as measured by the per cent frequency of CR's per block of extinction trials rose as the per cent of reinforced trials during the preceding training series was increased from zero up to 50 or 75%. Then there was a sharp drop in the function as the per cent of reinforced trials rose to 100. There was thus a maximum emission of CR's during extinction following 50–75% reinforcement. The simplicity of the extinction curves was complicated by slight rises during the first few extinction trials following a 25% and 50% reinforcement—a result reported by Humphreys (11) for 50% random reinforcement. The particular functions obtained were doubtless conditioned by the specific experimental procedures used, particularly by the intensity of the air-puff, the length of the training series, and the prevention of dark adaptation in the *Ss*. The acquisition and extinction results were in line with those of Grant and Hake (4), but the acquisition results differed from those of Humphreys, presumably because of differences in the above-mentioned features of the two experiments. Differences between present results and those of Grant, Riopelle, and Hake (8) were due largely to differences in frequency and magnitude measures.[2]

When the numbers of CR's per reinforced trial were compared for varying percentages of reinforced trials it was found that there was a distinct maximum in this function in the region of 50–75% reinforced trials, indicating that the effectiveness of each reinforced trial in evoking CR's was greatest for 50–75% reinforcement. This was true not only during extinction as has been noted by earlier workers (13), but also during acquisition—an unexpected, though possibly artifactual,[3] result.

The fact that resistance to extinction is maximal following 50–75% reinforcement may be interpreted in terms of two processes. On the one hand, the eyelid CR requires a certain minimal amount of reinforcement to appear consistently during acquisition and extinction and the 25% training schedule probably barely gives sufficient reinforcement. On the other hand, as has been suggested in various terms by Humphreys (11, 12), Mowrer and Jones (14), Sheffield (16), and others (13), extinction of a response

[2] In comparing results during reinforcement schedules involving varying percentages of reinforcement frequency data are to be preferred over magnitude data for the reason outlined in the previous section.

[3] It is obvious that the number of CR's per reinforcement during acquisition has an upper bound of 1.00 for 100%, 1.33 for 75%, 2.00 for 50%, and 4.00 for 25% reinforcement ratios.

depends upon discrimination of the shift from training to extinction procedure. This discrimination is made easily following 100% reinforcement, so that extinction might be expected to be relatively rapid following a 100% reinforcement schedule. These two processes taken together could account for a central maximum when resistance to extinction is plotted against percentage of reinforced trials. This interpretation received some support from a parallel of the present study (**5**), in which Humphreys' verbal conditioning (**12**) was used. The verbal response, presumably requiring less reinforcement, gave maximal resistance to extinction following 25% "reinforcement" when a plot corresponding to the present Fig. 3 was made.

Rational quantification of the two processes, reinforcement and discriminability of extinction seems to present major problems. The reinforcement function must be chosen from numerous possibilities, the exponential growth curve (**10**) being a prominent candidate. Discriminability might be attacked in terms of Shannon's mathematical theory of communication (**15**). Cases can be made for the logarithm of the relative "uncertainty," the logarithm of the ratio of the "uncertainties" of training and extinction series or other measures from Shannon's formulation as measures of discriminability of stimulus sequences. The proper measure of discriminability remains to be found, however. In merging the reinforcement function and the discriminability index for the total function, a simple multiplicative combination seems inadvisable since the extinction process apparently terminates at different times following different reinforcement schedules. A subtractive combination is therefore indicated. Other factors such as inhibition of reinforcement (**9**) and work decrement (**10**) might also be required. It is regretted that a completely plausible function cannot be set down at this time. The choice of alternative possibilities is being explored in current research at the Wisconsin laboratory.

Summary

The acquisition of conditioned eyelid responses during 92 training trials with 0%, 25%, 50%, 75%, and 100% reinforcement and during a subsequent 25-trial extinction series was studied in 70 human *Ss*, 14 with each procedure. Light was the CS, and corneal air-puff was the UCS. These were presented in standard time relationships under light-adapted conditions. When both reinforced and unreinforced trials were given, the order was random. The *Ss* were given "passive" or "neutral" instructions.

The essential findings of the experiment were as follows:

1. The per cent frequency of CR's during acquisition was clearly a function of the percentage of reinforced trials. In general, the per cent frequency increased as the percentage of reinforcement increased.

2. During extinction the per cent frequency of CR's was greatest in the 50% and 75% reinforced groups, dropping somewhat in the 25% and 100% groups. When plotted in terms of the number of CR's per reinforced trial, the higher efficacy of single reinforced trials in the 50–75% groups during extinction was found also to hold during acquisition.

The maximum about 50–75% reinforcement in the function relating resistance to extinction to the percentage of reinforced trials was interpreted in terms of two processes: (*a*) the increased response strength with increase in the number of reinforced

trials, and (*b*) the relatively greater ease of discriminating the occurrence of the extinction procedure if extinction follows a training schedule made up of a high percentage of reinforced trials.

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"REMINISCENCE" IN BILATERAL TRANSFER

ARTHUR L. IRION
Tulane University

AND

LEVARL M. GUSTAFSON
University of Illinois

The reminiscence phenomenon has been demonstrated in a variety of experimental situations and over a considerable range of learning tasks (3). This effect is most pronounced in motor learning situations where a number of investigators have obtained relatively large amounts of reminiscence (1, 2). Bilateral transfer of training has also been demonstrated in a variety of tasks which have been, of necessity, in the general area of perceptual-motor learning (6). Demonstration of the reminiscence effect in bilateral transfer would point to the need for controlling time intervals in bilateral transfer research. More importantly, such a demonstration would provide a technique for studying the locus of the reminiscence effect.

METHOD

Accordingly, a group of 40 undergraduate students at the University of Illinois served as *Ss* in an experiment which was designed to determine whether or not this type of reminiscence effect could be obtained. All *Ss* were right-handed males. The total group of *Ss* was divided, at random, into two equal groups. Each of these groups received ten trials of practice with the right hand upon the Koerth pursuit-rotor followed by ten trials of practice with the left hand. The general conditions of practice, the apparatus, and the recording system have been described, in detail, in an earlier paper (4). Suffice it to say here that the pursuit-rotor target moved in a circular path at a rate of 60 RPM in a clockwise direction. Each trial of practice was 25 sec. long and 5 sec. intervened between trials. The two groups of *Ss* learned under the same conditions except that Group I shifted from the right hand to the left hand (between Trials 10 and 11) with only the normal, 5-sec., intertrial rest. Group II, however, was allowed a 5-min. rest between Trial 10 and Trial 11 (the point at which the shift in hands occurred).

RESULTS

The results are shown in Fig. 1. All scores are reported in terms of the mean number of seconds per trial during which contact was maintained with the target. It will be seen that no important differences exist between the groups during the first ten trials of practice. Following the switch to the left hand, however, it is evident that Group II, which received the 5-min. rest, is superior to Group I, which shifted hands during the normal intertrial rest. The statistical significance of this superiority was assessed by means of an analysis of covariance technique. The cumulative performance for each *S* during the first ten trials served as the initial score while the cumulative performances during the Trials 11–20 were used as final scores. The resulting variance ratio is 11.83 for 1 and 37 *df*, a value which

TABLE 1

SIGNIFICANCE OF DIFFERENCES BETWEEN GROUPS FOR MEANS ON TRIALS 11–20

Trial	U-test (CR Value)	*p*	Trial	U-test (CR Value)	*p*
11	3.02	.001	16	2.05	.020
12	2.10	.018	17	1.73	.042
13	2.02	.022	18	2.13	.017
14	3.13	.001	19	1.37	.085
15	1.59	.056	20	1.46	.072

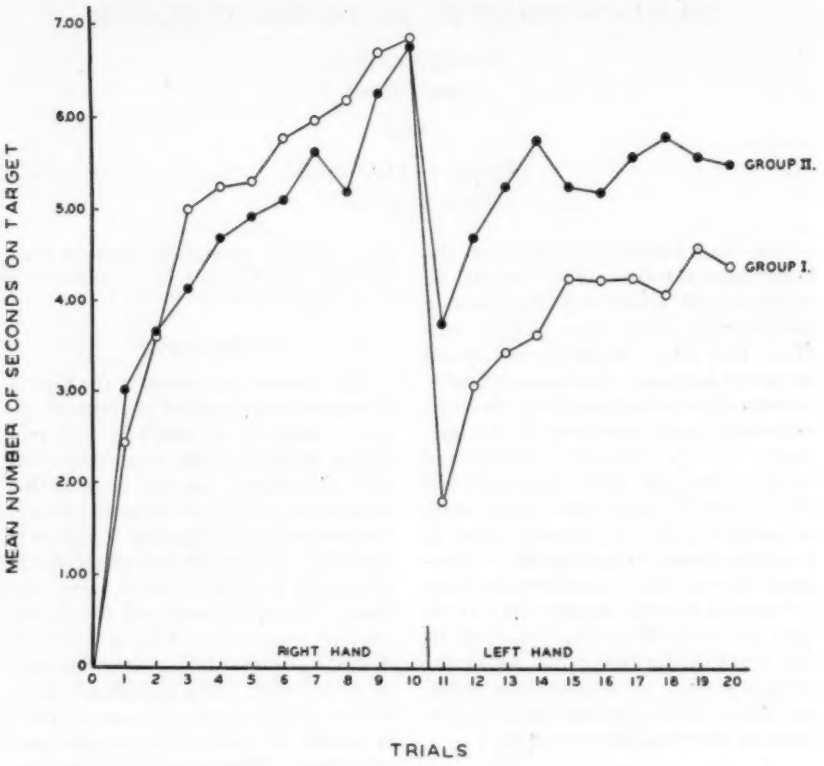

FIG. 1. Time on target during learning with right hand and test with left hand with (Group I) and without (Group II) 5-min. rest

is significant at beyond the 1% level of confidence. Significance of the differences between the groups on individual trials could not be assessed in this way, however, since the trial scores were not normally distributed. The Mann and Whitney (5) *U*-test was used for these comparisons, the results being shown in Table 1. It is evident that the beneficial effects of rest tend to be concentrated in the first few post-rest trials.

These data indicate that reminiscence does occur in the bilateral transfer situation and that the control of the time intervals in this type of research is essential. The data also indicate that this technique may be useful in research designed to determine the locus of the reminiscence phenomenon, since the results suggest that this locus may not be entirely in the effector involved.

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THE RELATIVE DISCRIMINABILITY OF SEVERAL GEOMETRIC FORMS [1]

ROBERT B. SLEIGHT [2]
The Johns Hopkins University

Many geometric forms have been studied by experimenters who have sought to order the world of shapes into a scale of efficiency. It is difficult to determine a consensus in this matter because the different researches are focused on only partially comparable stimuli and the criteria of discriminability employed have varied considerably.

Several experimenters have used extension of form field as a measure of discriminability (**2, 6, 9**). The results of these studies do not show complete agreement, however, on the ranking of the various forms employed. Another study ranked several geometric figures according to accuracy of reports when the forms were viewed at a distance (**1**). Two other investigators, measuring intensity thresholds for various forms, disagree upon the ranking of similar figures (**5, 8**). Two reports dealing with the visibility of distant targets indicate that, at least within certain limits, there is no difference in the probability of detecting several differently shaped targets of equal area (**3, 7**). Still other experimenters have used accuracy of peripheral discrimination as a criterion (**9, 11**). These experimenters agree on the relative efficiency of some forms, but disagree on the rankings of others.

One might go on sorting the literature into disagreements among comparable data and agreements among unrelated data, until one reaches the general conclusion that there can be no efficient ranking of geometric forms as an unequivocal abstract in itself. Whether, for example, a circle should be recommended over a square would seem to depend upon the total perceptual situation and upon the particular criterion of discriminability deemed most important. It seems appropriate, therefore, to investigate forms as they are "good" in particular classes of situations and for particular purposes rather than to search abstractly for "good" designs per se, as the Gestalt psychologists might insist.

It was the aim of this study to obtain some information on the relative discriminability of a number of individual geometric forms when they constituted a complex panorama with which *S* had to deal.

Two aspects of discriminability were considered in this study: (*a*) the efficiency of the various forms in terms of "sorting time" required, and (*b*) the "attention-getting" value of the forms as measured by the priority of *S*s' selections.

MATERIALS

The stimuli were six each of 21 different geometric figures, making a total of 126, constructed of black paper and mounted on 1½-in. clear lucite squares. Each figure was the maximum size which could be inscribed within a 1-in. diameter circle. For some figures somewhat arbitrary names were assigned. The forms used were the following and appeared, and were named, as

[1] This research was carried out under Contract N5-ori-166, Task Order I, between the Special Devices Center, Office of Naval Research and The Johns Hopkins University. This is report No. 166-I-55, Project Designation No. NR 784-001, under that contract.

[2] The author wishes to express his appreciation to Miss Grace Duvoisin for her assistance in the preparation of this report.

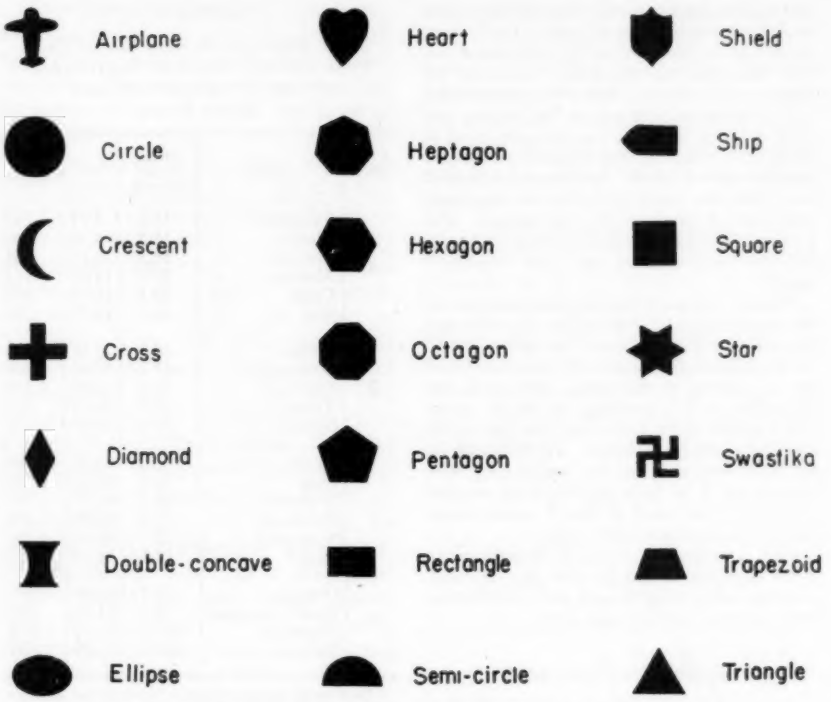

FIG. 1. Geometric forms for which discriminability was determined

shown in Fig. 1. In some cases, the configuration of the geometric form was arbitrary on the part of *E*; in other cases, the available literature was consulted to determine the most scientific construction. The rectangle and ellipse, for example, were constructed to conform to proportions which according to the research of Sleight and Mowbray (**10**) were discriminable on an absolute judgment basis from a square and circle, respectively.

A circular board, 25 in. in diameter and painted flat white, was used as a display background. A 4-in. border around this board was partitioned into 25 compartments for the placement of the forms as they were sorted by *S*.

PROCEDURE

The board was set on a table 30 in. from the floor. The 126 figures were spread out in random order and in various orientations across the face of the panel. Overhead room lighting was used. Timing was by means of a stop watch graduated in 1/100 min.

Before the display board was exposed, *S* was permitted to examine a sample of each kind of figure on the board. He was told to notice differences and similarities among figures and instructed on the following points concerning his task: (*a*) There were six of each kind of figure scattered on the board. (*b*) The essential task was to sort all six of a given kind into any one of the compartments along the side of the board as quickly and as accurately as possible. (*c*) Accuracy was to be emphasized before speed. (Preliminary testing had shown errors to be negligible, therefore it seemed advisable to attempt to eliminate them entirely by these instructions.) (*d*) Sorting was to proceed by picking up a given figure and depositing it, etc.

The sorting order of the first four trials was one of ascending difficulty as determined by *S*. That is, each *S* was instructed to sort first the kind of figure which, at a glance, seemed to "stand out" or seemed to be "easiest to sort quickly." All six of these were to be completed before another kind was sorted. The second kind was to be the second easiest and so on, so

that the last group of six figures on the board was that considered the most difficult. The *S* stood in front of the board and at the ready signal was given approximately one second to look at the display and decide the kind of figure he would sort. He began sorting at a "go" signal, and when the sixth figure (it was necessary for *S* to do his own counting) was deposited, *S* himself signalled when finished. At this point he looked away from the board until given the next ready signal from *E*—a matter of a few seconds. The *E* recorded the order in which *S* sorted the figures and the time between "go" and "finished" signals.

Trials 5 and 6 involved two variations on the above procedure: (*a*) The sorting order was predetermined and was different for each of the 21 *S*s, resulting in all figures being sorted in each of the 21 positions in the series. The order was announced to *S* by pointing out at the proper time a sample figure of the kind that was on the board and was to be sorted. (*b*) When six of any given kind of figure were sorted, they were replaced by *E* in approximately their original positions on the board so that *S* always began to sort from among all 126 items.

Sixteen male and five female *S*s participated in the experiment. All *S*s were given a visual screening test with the Bausch and Lomb Ortho-Rater and had normal visual acuity.

Results and Discussion

Table 1 shows the ranking of the figures in an ascending order of mean sorting time. The times ranged from approximately .11 min. for the first ranking figure to approximately 1.07 min. for the last ranking figure. More specifically, this shows that the time required to sort the hexagon was approximately ten times longer than that required to sort the swastika.

Since the similarities between the hexagon, heptagon, octagon, and pentagon seem on face value to be greater than those between the other figures used, these particular items may tend unjustifiably to magnify the differences among the data. However, even if these figures were ignored, the mean for the last ranking figure (the double-concave) would still be on the order of nine times as great as the mean for the swastika. In this respect it may be well to note the obvious limitations of any work done on relative discriminability of geometric forms. The octagon, heptagon, hexagon, and pentagon are configurationally similar figures and would logically be expected to be confused. That is, the relative discriminability of various forms is partially dependent upon the particular figures which constitute the stimuli. The question arises as to what result would have been obtained if, for example, only one of the multi-sided figures had been included in this study.

TABLE 1

Mean Times for Sorting Six of Each Form and Groupings by Significance of the Difference between Mean Times

Group	Form	Mean Time* (.01 min.)	*SD*	Skewness***
A**	Swastika	11.4	1.74	−.689
	Circle	15.2	4.75	.568
	Crescent	18.5	10.63	.761
	Airplane	27.9	13.24	.111
	Cross	29.1	16.74	.663
	Star	30.4	12.75	.972
B	Ellipse	40.0	26.80	1.220
	Rectangle	45.3	27.90	.806
	Diamond	47.2	22.29	1.238
	Triangle	53.7	28.22	.765
	Square	58.0	34.00	.679
C	Heart	66.0	52.72	.699
	Ship	70.2	34.35	1.150
	Semicircle	71.5	42.77	.793
	Pentagon	73.4	36.34	.495
	Trapezoid	77.3	46.65	1.086
	Shield	83.5	54.62	1.488
D	Octagon	94.7	54.88	1.088
	Double-concave	99.3	71.06	.895
	Heptagon	103.2	53.38	.972
	Hexagon	107.7	53.79	.731

* Each mean presented here is based on the last two trials for all *S*s combined ($N = 42$).

** Groups denote forms, each of which is significantly different from all other forms (at the 1% level) except those within the same bracket. For example, the swastika is significantly better than the ellipse, the ellipse is significantly better than the heart, and so on.

*** Skewness is calculated using the formula three times the mean minus the median divided by the standard deviation. In a perfectly symmetrical distribution the obtained value would be zero.

Significances of the difference between means were computed. Each type of figure was significantly different from at least 50% of the remaining 20 types of figures at the 1% level of confidence.

While there is no clear-cut basis upon which the figures may be divided, the existence of significant differences between means enables us to make groupings which may be of practical value. Gross categories of significantly different forms are shown in Table 1. These are somewhat arbitrarily broken down by groups, designated as A, B, C, and D. The figures in any given *one* of the four groups do not differ significantly from each other. This means, for example, that for any practical purposes to which the present data may be applicable, the swastika would not be appreciably better than the circle, the crescent, or any other figure listed in Group A. These divisions, however, are not unequivocal and without exception. While, for example, the swastika and circle are significantly different from all figures in other groups, the difference between the rest of the items of Group A and the ellipse of Group B is not significant. Other overlappings of this sort might be mentioned, but insofar as gross categories may be of service they are here made as indicated in Table 1.

When the data pertaining to the selection order of figures as determined by the *Ss* themselves are treated by means of a weighted scoring equivalent to the order of selection, they rank as presented in Table 2. The rank order correlation between this ordering and the ranking according to mean sorting time is .79. In view of this correlation coefficient and the fact that *Ss* were instructed to select first those figures which "stood out from the others," or "looked as though they could be sorted most quickly," it is evident that there is in practice an appreciable and direct relationship between the "attention-getting" value of these geometric forms and the ease and speed with which they can be handled.

It is notable that of the six "best" figures (Group A), only one or possibly two, the circle and the crescent, might be called "simple" in the sense in which this term has been used in threoetical discussions of form. The remaining four, the swastika, airplane, cross, and star were among the most "complicated" (considering number

TABLE 2

RELATIVE DISCRIMINABILITY OF GEOMETRIC FORMS AS DETERMINED BY (a) MEAN SELECTION ORDER, AND (b) MEAN SORTING TIME *

Form	Rank by Selection Order	Rank by Sorting Time
Swastika	1	1
Cross	2	5
Star	3	6
Airplane	4	4
Crescent	5	3
Diamond	6	9
Circle	7	2
Heart	8	12
Triangle	9	10
Double-concave	10	19
Semicircle	11	14
Shield	12	17
Rectangle	13	8
Ellipse	14	7
Ship	15	13
Square	16	11
Trapezoid	17	16
Pentagon	18	15
Hexagon	19	21
Octagon	20	18
Heptagon	21	20

* Rank order correlation = .79.

and direction of lines, angles, etc.) of the designs tested here.

When earlier researches are compared with the rankings of similar items from the study at hand, certain similarities and disparities become apparent. It would seem that the present study as well as the earlier researches supports the thesis that neither "good" nor "bad" are qualities intrinsic to form per se but an extrinsic evaluation that may be applied to any given form depending upon the total situation being considered and the purpose to be served.

Summary and Conclusions

The *Ss* were required to sort a number of geometric forms presented in a complex visual display. There were 21 kinds of figures with six of each kind.

1. Discriminability as measured by sorting time was approximately ten times faster for the first ranking than for the last ranking figure.

2. On the basis of significance of differences between mean sorting times, it was possible to identify four groups, in any one of which the figures were approximately equally discriminable. The most discriminable group was composed of the following forms: swastika, circle, crescent, airplane, cross, and star.

3. A high positive correlation was found between the ranking of figures according to sorting time and ranking based on *Ss*' order of selection of items according to their "attention-getting" value.

4. Of the six best figures in terms of speed of sorting time, only one or two might be considered "simple" in the sense that this term is used by Gestalt psychologists.

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